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# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*On the Importance of the Uterine Ebb as a Factor in Pelvic Surgery.*[1] By HORATIO R. STORER, M.D., of Boston, U.S., former Vice-President of the American Medical Association, and Member of the Medico-Chirurgical and Obstetrical Societies of Edinburgh.

UPON returning to England from a four years' absence upon the Continent, I was informed by Dr Marion Sims at Sheffield, during the session of the British Medical Association, that a project, often discussed during 1870-71 between myself and my then associates, Drs Warner and Bixby of Boston, had at last been carried out, in the establishment of the American Gynæcological Society. At Professor Alexander Simpson's, in Edinburgh, I have since been shown the prospectus of the Society, and perceiving that papers are solicited for the first annual meeting, it gives me pleasure to add my mite towards elucidating one of the important problems for whose settlement the Society has been formed.

Practitioners of experience in pelvic surgery will have been struck by the difference in result of operations upon cases apparently similar as to character of the lesion, age and constitutional condition of the patient, method of operating, the preparatory and prophylactic measures resorted to, and those attending the periods of wound and of convalescence. With every care, and with the utmost skill on the part of the operator, some cases will as it were persist in going wrong, while others will rapidly and surely recover under the most unfavourable circumstances. It has long been my belief that this is not wholly a matter of accident, nor of

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[1] The above paper was presented to the American Gynæcological Society at its first annual meeting (New York, 15th Sept. 1876), and by unanimous vote of the council, Drs Fordyce Barker, Marion Sims, and Munde of New York, Byford of Chicago, Atlee and Goodell of Philadelphia, Parvin of Indianapolis, and Lyman and Chadwick of Boston, received the thanks of the Society, as communicated by the secretary, Dr Chadwick. By the kindness of the Society the writer has been permitted to decide where the paper shall be published.

"personal equation" among operators; and only recently Dr Thomas Keith, who has had, unless we except Mr Spencer Wells, the most continuous runs of recoveries after ovariotomy, has confirmed my opinion by expressing to me his conviction that the true secret of ensuring convalescence had not yet been reached. The selection of cases, and employment of skilled assistants and nurses, no doubt go far towards compassing a cure, but even with these a patient will often perish when everything has seemed in her favour. To consider, then, whether there may not be essential points as yet unappreciated or lost sight of, may well engage the Society's attention.

It has seemed to me, on studying with some care the statistics of pelvic operations common to the two sexes, and the details of individual histories, that there existed a difference, alike as to success in isolated cases and in series more or less extended in number. There are necessarily but few of these common operations, the most frequent perhaps being for strangulated hernia and for hemorrhoids and anal fistula; for in vesical calculus all the conditions vary so greatly in the two sexes, and extraction by dilatation is ordinarily so easily performed in the female, that the other methods adopted, whether cutting or crushing, are hardly to be compared. Nevertheless, it is my impression, that allowing for the difference arising from previous habits of life, there are to be detected variations in the rate and certainty of convalescence explainable by sex alone. In the male, the general constitutional condition may be said to be in stable, and in the female during a great part of her life, and owing to constant and special physiological causes, in unstable equilibrium, and therefore more prone at times to take on undue inflammatory action, and at others less sure to undergo even that degree of it, without which convalescence after an operation can hardly be hoped.

Such being the fact, it is not unreasonable to search for the disturbing factor of surgical pelvic averages among influences essential and functional, and not, as has been done hitherto, either to consider it personal to the operator, or peculiar to the individual patient—in other words, an accident. This point, so far as I am aware, has hardly been dwelt upon with emphasis by writers, and to the extent to which I am inclined to develop it, it is practically ignored by surgeons.[1]

There are no less than two general conditions that require attention before grave pelvic operations are attempted upon the female, namely, that of the body and that of the mind. In most cases of men we may safely enough leave the latter of these conditions to take care of itself, though all will acknowledge that convalescence is frequently hastened by the mere existence of cheerfulness, fortitude, and hope, and that it is as often retarded or prevented by apparently so trivial a fact as their absence. *A fortiori*, these influences for or against recovery are exerted with infinitely greater certainty and power in women, with whom they are not so much the effect of temperament, surroundings, confidence in the attendant, or other fortuitous circumstances, as essential to their sex. Scarcely a healthy woman in whom the observer cannot after a while detect traces of this phenomenon; while in the insane, in whom the tendency may be uncontrollable, either by the will or by the checks of ordinary domestic and social life, the mental disease often plainly evidences, by its exacerbations and other changes, the time, almost the very day indeed, of the personal month.[1] In both sane and insane, these personal time-variations are easily enough to be ascertained and made practical use of. They will be found to constitute an important, and perhaps the most important, factor in pelvic surgery.

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[1] Since the above was sent to America, the writer has been happy to learn from Mr Spencer Wells (Nov. 1876) that he and other London surgeons were more and more acting upon the principle alluded to, in practice.

If this is so true concerning the mind, one cannot wonder that, to the general physical, or rather physiological condition, upon which the rise and fall of the mental barometer so absolutely depend, I have been led to attribute a great influence in deciding the result of surgical procedures. The menstrual molimen is something more than a mere technical phrase. It is a reality, that not merely determines, and is evidenced by, the normal catamenial discharge, but that affects reflexly, and probably even more directly, the woman's whole being. The respiratory system is materially influenced, alike in the amount and character of its work, as Andral and Gavarret long ago proved. The circulatory rhythm, and its general and local force, are all for the time altered, as can be seen by the sphygmograph and the thermometer. Both assimilation and waste share in the disturbance, if this term can be properly applied to a phenomenon so natural; while extreme nervous excitement reveals itself by its direct results, the various changes to which allusion has now been made.

To consider the catamenial function as wholly a local one in effects and in origin, as many general practitioners and even gynæcologists seem still, both in theory and in practice, to be inclined to do, is not merely scientifically wrong, but it has doubtless often occasioned the death of a patient. Were the ovaries the only seat of the molimen, it were impossible to account for the thousand localities of vicarious menstruation. To say that this is merely a diseased or weakened tissue replacing the uterine suppression may be a true statement of fact, but does not explain its cause. The cases pronounced so important by Brown-Séquard, that I have myself put upon record, of uterine menstruation after the removal of both ovaries, and of vaginal menstruation after the removal of the entire uterus as well as both ovaries,[1] and subsequent reports of the kind from the experience of other operators, settle beyond doubt, that while the ovaries are the normal seat of the periodical explosion, the influence which occasions it is one that affects with more or less distinctness the entire organism. Putting aside all momentary and accidental variations of the heart's action, there normally occurs, however we may phrase it, and during the woman's whole menstrual life, a regular and constant disturbance that we call periodical, but which is none the less unceasing. The sun's rising is periodical, but it also sets. Spring periodically comes, but so also do autumn and winter. We must take care that we are not misled by a single and the most patent phase of the catamenial circle, and forget that there exists even here a low as well as a high tide, and that while there regularly occurs a condition of slowly increasing local congestion and hyperæmia, there as regularly takes place a distinct uterine ebb, whose influence as a factor in pelvic surgery has not sufficiently been recognised.

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[1] To the importance of the above and collateral points the writer long ago called the attention of the profession in a series of papers upon the causation, course, and rational treatment of insanity in women, read before the American Academy of Arts and Sciences, the American Medical Association, and elsewhere, the first of which was published in the Boston Medical and Surgical Journal early in 1864, and others, in the Transactions of the American Medical Association for 1865, etc., thus antedating by several years the communications of Prof. Louis Mayer to the Obstetrical Society of Berlin.

That the consecutively occurring general and constitutional conditions now indicated should have their importance for surgeons ought not to have seemed improbable, even had not theory in this instance proved the goal instead of starting-point. It is not logical to closely compare things that are in some respects dissimilar, but roughly to do so will at times assist one's purpose. Physiological periodicities are different from ordinary physical phenomena, and yet in some respects they resemble them, and they are governed by analogous laws. In our language, for default of a better nomenclature, we have to allude to the tides, and speak of a uterine ebb. Were not the correlative expression already attached to the condition of menstrual culmination, we might as properly call the period preceding this the uterine flow. They have their separate safeties and their separate dangers, to be seen in different states of health, and in different stages and varieties of disease. To several of these I have already called the attention of the profession in former years. Whatever the ultimate decision in the ether-chloroform controversy, the fact that I first clearly pointed out many years ago,[1] that anæsthesia, even if performed without danger, may be borne during childbirth, has been accepted as true, and also the theory by which it was explained, normal parturition occurring at a menstrual period, when the lungs and indeed general system are under very different conditions than during the interval. Like day and night, with their plainly perceived effects on health and on life, so the uterine ebb and the uterine flow. In the night, and not through chance, most births occur, and most deaths; and so, during the menstrual molimen, and at the menstrual period resulting from it, a patient with fibroids or epithelioma is most likely to die of hemorrhage.

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[1] This was in 1865. (American Journal of the Medical Sciences, Jan. 1866, p. 119.) Cases of menstruation after double ovariotomy, by the writer, are reported in the Canadian Medical Journal for 1868, and the American Journal of the Medical Sciences for January of the same year. Dr Atlee of Philadelphia has mentioned in his work upon the Medical and Surgical Diseases of Women (1867) that he had twice noticed menstruation where both ovaries had been removed at different periods, and twice where it had been done simultaneously. Reeves Jackson of Chicago, Peaslee, Meadows, and others, have subsequently reported similar cases. Dr Battey, of Georgia, in his paper upon Normal Ovariotomy, read before the Medical Association of that State, has adverted to their physiological interest. (Atlanta Medical and Surgical Journal, 1873.)

Before appreciating the importance of this doctrine in all its entirety, it had occurred to me that for certain operations it might be better to select the menstrual period itself, on the ground that the natural bloodletting then taking place and continuing after the operation would likely tend to ward off peritoneal or other pelvic inflammation. In two or three instances of ovariotomy, I accordingly did what other operators, deterred by an indefinite fear that had found no expressed explanation, seem then not to have ventured, and I made the section during the catamenial period.[2] These cases have been published, and, if I recollect rightly, they each recovered. I now believe that I had taken a step in the right direction, but think that the argument that had led me to do so, though so far correct, was incomplete. The truth now seems that, with the commencement of menstruation, the uterine ebb, or catamenial reflux, has practically begun. The menstrual nisus passes into abeyance when it has occasioned the fracture of the Graafian vesicle and detachment of the ovum, accompanied as this may be either by rupture of uterine capillaries, simple transudation of blood or of liquor sanguinis, or effusion of that stained mucus which, as a periodical leucorrhœa, so frequently unrecognized by the practitioner, precedes the apparently exceptional conceptions, occurring before any marked establishment of puberty, during the amenorrhœa of lactation, or after the menses have finally ceased. The usual persistance of the catamenial discharge for two or three days in no way contravenes this statement. At high tide the water is for a time preserved at a certain level, but the *progressive* force that had steadily lifted it has ceased. To revive the old discussion as to whether the catamenial fluid is chiefly venous in its character, or is arterial blood that has been defibrinated during its cervico-vaginal passage, or both, would in nowise affect the question.

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[1] In a paper read before the Massachusetts Medical Society in 1863, and published in the Boston Medical and Surgical Journal for that year. At the time the writer was living in Edinburgh, during 1854 and 1855, Sir James Simpson, though he may have suspected the fact, had not arrived at its full explanation, and no clue was given to it in his "Memoirs and Contributions," edited in the latter of these years by Dr Priestley and the writer, under Simpson's direct supervision.

[2] The first of these cases was at Chelsea, Mass., in 1867, and the operation was performed in the presence and with the kind assistance of Mr Spencer Wells. It was also the original instance of the writer's procedure, not approved by Mr Wells, of "pocketing" the pedicle, especially adapted for cases where too short to bring out externally, by including and closing in its extremity between the internal and external limits of the abdominal wound. (American Journal of the Medical Sciences, Jan. 1868; New York Medical Record, Jan. 1868, p. 519; Journal of the Gynæcological Society of Boston, Sept. 1869, p. 147.) In a subsequent case, where there was no pedicle at all, the writer thus pocketed an angle of the uterus itself, and the patient recovered, and subsequently did well. (Loc. citat. p. 137.)

The uterine ebb, again, like that of the tide, is most powerful during the mid interval between its inception and the negative or anaemic point of perfect uterine rest; but as it is well to allow for the week or half-week immediately subsequent to a surgical wound, the days of which are each of them and always more or less critical and anxious ones, danger may be avoided by operating at an earlier period than might otherwise seem necessary. Nature herself gives us an indication through the phenomena of what we call natural labour. This occurs, as has previously been said, at a menstrual period—the tenth—but it is ordinarily completed some hours, or a day or two even, after the molimen itself has undoubtedly ceased. Labour pains are not to be confounded with the menstrual force which has preceded them. It is this that excited them, it is true, but their completion has been reflex and indirect. Fatty degeneration had occurred in the uterine walls and in the placenta, and their separation from each other thus been prepared for. Uterine contraction once initiated, though but in the slightest degree, and though the os uteri be still undilated, the commencing cylindrical wedge of the amnion at once acts as a mechanical stimulant and reflexly excites to a greater degree of contractile action; while the utero-placental effusion of blood, though but to a single drop, from the rupture of but the smallest capillary, produces the accelerating effect—soon, moreover, exerted by the fœtus itself in the no longer distended cavity—of a foreign body which must be expelled. The birth over, the immediate hæmorrhage ceased, and the lochia, or simple drain from the closing placental wound and escape of fat globules and disorganized uterine cells through the general mucous membrane, having set in, the uterine ebb is more distinctly present. It is then that Nature gives us a second and still more manifest indication by the establishment of lactation; a process involving a sudden and fundamental divergence of the whole stress of the circulation from an internal to an external organ, which, in the case of a first labour, has as yet been but partially developed. Intended, no doubt, to relieve the womb by lessening the chance of secondary hæmorrhage, to permit the more rapid progress of involution and discharge of effete uterine tissue, and to tend to prevent subsequent mechanical displacement by thus lessening the weight of the organ, lactation still faces us as a physiological revulsion equalling that ever occasioned by surgery. The removal of a limb causes a tremendous shock to the heart's action, and to the processes of nutrition as well. Were three days allowed for the change, and the bloodvessels prepared by previous weeks of shrinkage and anastomosis, the shock would still be a great one; but here it is as though a limb were suddenly removed, and then, even allowing for the development of the breasts during gestation, almost as suddenly reattached to another portion of the body, thereby doubling, instead of lessening, the strain upon the nervous and arterial centres. It could hardly have been possible save during the uterine ebb.

And again, Nature gives us a third pertinent lesson in what we see during the progress of gestation. A pregnant woman may bear a violent fall or a severe blow with no ill effect, and the same woman may miscarry upon the slightest fright or muscular effort. Upon consulting her almanac, it will be probably found that in the latter instance she was at the uterine flow, in the former at the uterine ebb. We may wish, for good reason, to induce premature labour, and, to our surprise, may again and again introduce the sound fairly to the fundus, sweeping the membranes from the uterine wall, but without the slightest result; we shall then discover that we have selected the uterine ebb. We wait for a few days till the flow has again commenced, and we find the labour induced by the most trifling exciting cause.

Yet further, the abortive habit is, to many medical men, practically incurable. They appreciate its disastrous effect upon the woman's general health, they recognise its regularity of occurrence, and they know just when to expect the final catastrophe. How few, however, have detected its dependence upon the masked but effective menstrual molimen, or discovered that preventive measures may be successful if resorted to during the period of the uterine flow. One waits till the uterine ebb has fairly commenced, and, because then often too late, adopts the erroneous opinion that threatened abortions, even if occasioned by uterine or placental disease, cannot be arrested or averted. That they are occasionally checked in a blundering way, even after hæmorrhage has commenced and dilatation of the os begun, does not invalidate what I have said. The abortion usually results from the morbidly intense menstrual molimen, and the true indication is, by appropriate measures, of which we have already many, to control the molimen itself, rather than to delay till what we are trying to avoid has already practically taken place.

And still again, the extraction of a tooth during gestation, or even the filling of one, if at the uterine flow, may result in the expulsion of the fœtus; while the severest pelvic operations, as the removal of vaginal tumours, and even of ovarian cysts or abdominal fibroids, which may be thought necessary to preserve the life of either mother or child, may be, and have repeatedly been, performed without curtailing the progress of gestation. Two chances to one that these successful operations, though performed at haphazard and without suspicion of the fact that gave them their issue, were at the uterine ebb.

The above are all of them points that, thus explained, will come home to every practitioner. The conclusion to be drawn from them is, that for pelvic operations, all other things being equal, it is better to select the week immediately following the cessation of the catamenia, or, as nearly as can be ascertained, corresponding with what would have been this time had they appeared.

## ARTICLE II.—*On Excision of the Joint between the Os Calcis and Astragalus.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary and Lecturer on Clinical Surgery.

EXCISIONS of certain of the bones and joints of the foot for the removal of disease are now well-recognised operations. The able lectures of Mr Hancock on this branch of surgery have done much to direct attention to these operations; and I think I am justified in stating that the most recent experiences confirm the advisability of such proceedings in suitable cases.

That the entire ankle-joint, astragalus, os calcis, and other of the tarsal bones may be removed alone or together with a good result has been surely proved by the operations of Wakley, Liston, Hancock, Teale, Holmes, Sayre of New York, my colleagues, Mr Lister and Dr Watson, and other surgeons of eminence. I have myself excised the entire ankle-joint in four cases, and the os calcis and other tarsal bones in many more, with most encouraging results.

There is still some difference of opinion among surgeons as to the advisability of these operations; and the two principal objections which have been brought against them are—the risk of inflammation following the operation and spreading to the other bones and joints of the foot; and the uncertainty that the result of the operation will be a complete cure of the disease and a useful foot. I have, however, confidence that, by a proper selection of cases, with the careful use of antiseptic treatment, by not delaying operative interference too long, and by the employment of the bloodless method of operating, so as to ascertain the exact extent of the disease, these risks and objections will disappear, and that the conservative surgery of the foot will, in many instances, be the rule, and not, as now, the exception.

In this paper I intend to confine my remarks to the conservative surgery of one joint of the foot, namely, that between the os calcis and astragalus. As in the other joints of the foot, disease may commence in the neighbouring bones and affect this joint secondarily, or it may originate in the joint textures.

Pathological and clinical observations have taught me that there is a considerable class of cases in which disease originates in, and is for a time quite confined to, this joint; and in my own small pathological collection I have several well-marked examples of this condition. The liability of this joint to be injured, and its strong ligaments stretched or torn, in violent sprains of the foot, is sufficient explanation why disease should originate in it. That disease does originate in the articulation is a fact known to practical surgeons; and, in confirmation of it, I may briefly quote a few authorities:—

Mr Syme,[1] in his remarks on amputation at the ankle, says, "Caries is frequently seated in the articular surfaces of the joint between the astragalus and os calcis, where it may seem to be of limited extent and occasion no formidable symptoms, but nevertheless exhibits its characteristic obstinacy, and resists all means of remedy except amputation."

Holmes[2] says, "Disease of the tarsus has very commonly its starting-point in the joints between the astragalus and os calcis, as Sir B. Brodie long ago pointed out, though it ordinarily begins in the structure of the bone."

Bryant[3] remarks, "In the joint between the astragalus and os calcis, disease, however, may be found alone; and I have often been disposed to think that such disease is commonly the result of some lateral sprain of the ligaments that held these bones together; lateral sprains of the foot being more likely to be felt at this articulation than at the ankle, the astragalus being held so firmly between the malleoli."

Although, therefore, the existence of disease originating in and confined to this joint has been long recognised by surgeons, no attempt has, so far as I am aware, ever been made to act upon the diseased joint alone, and remove by excision its affected surfaces. Some authors advise that, in disease of this joint, either the entire os calcis or astragalus be excised, and the remaining diseased surface gouged away. Others, as the late Mr Syme, advise, and have largely practised, amputation at the ankle for this disease.

The apparent difficulty of obtaining free access to this joint, and of removing its surfaces, is, no doubt, the reason which has hitherto prevented surgeons performing re-section of it; but having recently successfully practised a thoroughly efficient method of excising this articulation, I venture to hope that, in describing this method, I am doing some little to promote the progress of practical surgery.

The importance of recognising and treating early disease originating in this articulation, especially after suppuration has taken place, must be acknowledged, for, owing to the situation and connexions of this joint, there must always be a peculiar risk of the gradual implication of the surrounding bones and joints.

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[1] Observations in Clinical Surgery.

[2] Surgery, its Principles and Practice.

[3] The Practice of Surgery.

Having carefully studied the anatomical relations of the joint under consideration, I found that its entire extent could be best exposed without injury to surrounding structures in the following way: The foot having been placed in the extended position, and resting on its inner aspect, an incision, commencing about an inch above the tip of the external malleolus, and carried along its posterior border in a curved direction to the calcaneo-cuboid joint, thoroughly exposes the posterior and external portion of the joint, when the peroneal tendons have been drawn outwards, and some ligamentous bands divided. This incision will be found to run along the inner border of the tendon of the peroneus brevis muscle. The anterior and internal portion of the joint can then be exposed by placing the foot still in the extended position on its outer aspect, and making an incision from the tip of the internal malleolus along the course of the tendon of the tibialis posticus, as far as the prominence of the scaphoid bone, drawing forwards this tendon, and carefully drawing backwards the other tendons and the posterior tibial vessels and nerve. By making the first incisions through the skin and cellular tissue only, and so ascertaining the exact position of the tendons likely to be injured, then cutting down through the periosteum to the bone, and with a periosteal scraper separating to a sufficient extent the periosteum, together with all the other superficial tissues, there is little risk of injuring any of the tendons or other important structures.

Both aspects of the joint having in this way been exposed, it will be found that by means of the chisel and mallet the articular surfaces can be easily and accurately removed, the posterior portion being removed through the external incision, and the anterior portion through the internal one. Should there be any disease in the hollow or fossa between the two articular surfaces, it can be readily reached and removed with the chisel or gouge through either of the incisions.

I have now to relate the following case, which is not only a proof of the possibility and success of this operation, but is also an illustrative example of the form of disease in which I think that excision may, instead of amputation or removal of an entire os calcis or astragalus, be employed.

Catherine K., age 18, was admitted into my wards in the Infirmary on the 20th of March of this year. Her history was, that about two years previous to her admission she severely twisted her right foot. From that time she suffered more or less pain in the region of the joint between the os calcis and astragalus, with swelling of the foot below the ankle. She was treated by rest and the application of warm fomentations, tincture of iodine, and other remedies, but with only very temporary benefit. As her symptoms were becoming steadily worse, she requested to be admitted into the Infirmary.

On admission the symptoms of disease in the calcaneo-astragaloid articulation were well marked. These symptoms were swelling on the lateral and posterior aspects of the foot immediately below the malleoli, tenderness on pressure, and pain when the foot was firmly grasped, and moved laterally and rotated. The ankle-joint was healthy, and the anterior tarsus and portion of the foot were free of disease. The first treatment consisted of complete rest, and the application of soothing means to relieve the pain. Shortly after this a small abscess formed on the external aspect of the joint, and it was opened and the wound dressed with anti-septic precautions. The wound was healed in about a month, and the foot, ankle, and leg were then confined in pasteboard splints and a starch bandage. With this appliance she was sent to the Convalescent Hospital for six weeks, but on her return little change had taken place in the symptoms. The splints were reapplied, and she went home, but returned again on the 20th of July, as the disease was no better, but rather worse. The swelling and pain were now more marked, and in addition there was a feeling of deep fluctuation over the outer aspect of the joint. On account of this aggravation of the disease she was readmitted, and on the 29th of August I exposed the lateral aspects of the joint in the way I have described, and finding suppuration with caries of its articular surfaces, more particularly of those of its posterior portion, I removed with the chisel the entire articular surfaces of the joint —that is to say, the articular surfaces on the upper aspect of the os calcis, and the corresponding surfaces on the under aspect of the astragalus. The interosseous ligament was softened and in part destroyed. After the diseased surfaces had been removed, a drainage-tube was passed completely through the joint, and its open ends allowed to project at either wound. The operation was performed with antiseptic precautions, and the usual antiseptic dressing was afterwards applied and continued until the 10th of October, when the boracic-acid lotion was substituted for it. The patient's progress after the operation was excellent, and the wounds gradually contracted, and were soundly healed on the 22d of November.

At this date the appearance of the foot was natural, and the movements at the ankle-joint were free and perfect. The patient could bear good weight upon the foot, and it promised to be in time quite strong and useful.

## ARTICLE III.—*On Rupture of the Urinary Bladder and its Surgical Treatment.* By J. A. MACDOUGALL, M.D., F.R.C.S., Carlisle.

MY object in writing the following paper is to draw attention to two facts which I regard as of much importance to the practical surgeon—the first, that rupture of the urinary bladder is not always the necessarily fatal injury which many regard it; and the second, that its treatment, now commonly an expectant one, may with good reason be made more active, and more likely to be followed by success. To substantiate my first observation, I give in detail the records of two cases in which I believe this accident occurred, and in both of which recovery happily followed.

CASE I. was under the care of Dr Thom, of Brampton, and can best be told in his own words:—"R. B., a farmer, living about three miles from Brampton, had been drinking a good deal at market, left for home at 3 P.M., but stopped for a little while at a public-house on his way. In passing through a gateway he was thrown from his cart, which was heavily loaded, and the wheel, he says, passed over his abdomen. He lay for about an hour, was then found, and carried home at 8 P.M. I saw him two hours afterwards. He was on his back in bed, still intoxicated, but complaining of great pain in the belly and of great desire to pass water, which, however, he had not been able to do. On being questioned, he stated that he had last made water at half-past two o'clock. There was a bruise extending from the left crista ilia across the pubes; a deep abrasion on the dorsum of the penis; and a fracture of the ilium, not extending into the true pelvis. There was no blood at the orifice of the urethra. A flexible catheter was introduced without a stylet, but was withdrawn, as it would not enter the bladder. There was no blood upon it when it was withdrawn. It was easily introduced without a stylet, and was felt to pass along the prostate when examining per rectum. No urine flowed at first, but when pressure was made over the bladder, six ounces came away deeply coloured with blood. The catheter did not move freely in the bladder, and it required constant pressure to get the urine away. His pulse was 88 and weak, face flushed, countenance not expressing much anxiety. He threw himself about in the bed in the most violent way, being still very drunk. I injected morphia subcutaneously, and saw him again at 3 A.M. next day.

His pulse was then 100, weak, abdomen more distended, but no bladder distention. Two ounces of urine were drawn off with the catheter less bloody than the last. Morphia again injected. He was next seen at 11 A.M. by Dr Wotherspoon, who again introduced the catheter, withdrawing a less quantity than before. His pulse was then 120.

Mr Page saw the patient with me to-day (Thursday) at half-past one. His pulse was then 140. Respiration 40 in the minute, not entirely thoracic, but nearly so. Abdomen very tender, much distended and tympanitic, except for two inches above the pubes, where it was dull.

Mr Page confirmed my opinion that the bladder was ruptured, and the following were our reasons for thinking so:—

*First*, The patient's bladder was probably much distended at the time of the accident, as he had been drinking freely, and had made no water for some hours.

*Second*, The wheel of the cart had passed over the region of the bladder with sufficient force to fracture the ilium.

*Third*, Only six ounces of urine came by the catheter when it was first used, and only flowed when pressure was made above the pubes.

*Lastly*, There were signs of severe peritonitis.

Mr Page advised the retention of an elastic catheter in the bladder, the steady administration of opium, and perfect rest, which, with a now sober patient, were more easily managed.

Our opinion was, that the case was an almost hopeless one, but Mr Page remarked, "that he seemed to bear the injury remarkably well, there being no marked evidence of abdominal shock." On Friday he was rather better, on Saturday much improved. Pulse, 100; respiration, 20. Abdomen less swollen, and dulness above the pubes all but disappeared. The urine continues to flow freely through the catheter, which was left without a plug.

The catheter was retained in the bladder for a week longer, and although it was frequently changed and washed, the urine became purulent and ammoniacal. This was now, however, his only symptom. When the instrument was forcibly removed he made urine freely and well. Its purulent character soon disappeared, and his recovery was quickly perfected.

CASE II.—John Bland, a waggon driver, æt. 23, a man in good health and of temperate habits, was admitted into the accident ward of the Cumberland Infirmary on the 30th of August last. Late in the afternoon of the day previous, while in charge of an empty waggon, one of the horses attached took fright, and, striking him, knocked him down. Two wheels passed over him, one over the lower part of the abdomen, the other over his right arm. The arm, badly fractured, became at once painful and powerless, but for a time his other injury gave him little trouble. He was taken to a bonesetter, who reduced and treated the fracture, then placed in a cart, and driven a somewhat long distance to his home. Just before reaching it, and after an interval of fully three hours from the receipt of his accident, he was seized with pain in the belly. With this came urgent and intense desire to make water, but all attempts to do it failed. Many miles removed from medical assistance, and feeling sick and weary, he retired to rest, and, strangely enough, he slept fairly well during the night. With morning, however, came an aggravation of all his symptoms, and he sought the advice of Dr Robertson, of Penrith. He, learning from him his total disability to pass urine, used a catheter, but only succeeded in removing four ounces. This was quite twenty hours after the last time he had made water, and what he withdrew was deeply tinged with blood. The serious nature of the case was but too apparent, and he recommended his admission to the Infirmary. Bland entered the ward while I was making the morning visit, and so freely and apparently easily did he walk that I never suspected his condition was so serious. What of his history I have now told he then related, and he was immediately sent to bed.

The report of his condition as then observed is copied from the Ward Journal—"The patient is a strong healthy-looking man, well nourished and well developed. His integument is warm and dry; his temp. 99°. He has an anxious, pinched expression of countenance, and marked nervous twitching of the muscles of the face. On removing the splints from his arm, he was found to have a badly comminuted fracture of his humerus. Examination of the abdomen shows percussion dulness absolute in the right iliac region, the hypogastric region, part of the umbilical region, and to a less marked degree in the left iliac region. The lightest touch gives him pain; pressure he cannot bear. This tenderness is most marked towards the right iliac fossa. A full-sized catheter with a short curve was carefully introduced, and about one ounce of bloody urine withdrawn."

The history and the examination left little doubt that he had rupture of his bladder. That the matter might be rendered if possible more certain, he was put under chloroform, and passing my left hand into the rectum, well beyond the prostate, with a short-beaked catheter in the bladder, the empty and contracted condition of the viscus was readily demonstrated. In order to remove the urine effused into the peritoneal cavity, a No. 2 aspirator needle was introduced in the centre line an inch above the pubes. Bloody fluid escaped slowly through it, and when nearly five ounces had been withdrawn, the instrument, partly due to the consistence of the fluid, partly to some fault in the suction apparatus, struck work.

This was matter for regret, as the condition of the patient was such that the removal of the fluid was imperatively demanded. Not only had he already very evident signs of abdominal inflammation, but some symptoms he had suspiciously indicative of uraemia. Hot fomentation was applied over the abdomen, and a morphia suppository administered. Chemical examination proved the fluid removed to be urine highly charged with blood. I visited him again some hours later, and found him markedly worse.

Vomiting, which had occurred for the first time shortly after I had left him, had rendered the pain more severe, and the vital depression was greater. Two modes of procedure now suggested themselves, either to use the largest-size trocar of the aspirator, so as to give ready vent to the accumulated fluid, or having freely opened the peritoneal cavity, to sponge it thoroughly out, and make an attempt to reach and close the wound in the bladder. Mr Page, who now saw the case with me, agreed that, failing a satisfactory result from the first procedure, we should adopt the second. The trocar was accordingly introduced obliquely downwards and backwards, close above the pubes, the aspirator affixed, and by it twenty-one ounces of fluid were withdrawn. As it flowed the man expressed relief, and ere the operation was finished, his pulse, which had previously been depressed and irregular, became sharper and more steady. All abdominal dulness had now disappeared, and the acute tenderness seemed even less. A soft catheter was passed and retained. Hot fomentations were applied to the abdomen, the grain morphia suppository repeated, and orders given that he should have one every four hours. Ice and weak beeftea he was to have *ad libitum*.

*31st Aug.* Pulse 120, temp. 100·2°; has slept fairly well during the night, and does not complain so much of pain. The urine, which still contains much blood, passes freely by the catheter. The tenderness of the abdomen on pressure is still extreme, but there is now no dulness.

*Vespere.*—His pulse and temperature have risen, and it was ascertained that no urine had passed by the catheter for some hours. With the idea that the instrument had become blocked, it was withdrawn, washed, and re-introduced, but with no better result. Percussion revealed a little return of the dulness above the pubes: and fearing that the urine was again finding its way into the peritoneum, the house-surgeon, Mr Spence, used the aspirator, and withdrew several ounces of sanguineous fluid. The removal of this, which examination proved to be largely sero-purulent, was followed by benefit, for in a short time the urine again passed guttatim by the catheter.

*1st Sept.* Pulse 120, temp. 99°. For fully half an inch above the pubes there is still marked dulness, which shades off towards the left iliac fossa. The pain and tenderness are less, but the patient is slightly jaundiced. *Vespere.*—Urine, which is much less bloody, escapes freely. Pulse 124, temp. 102°.

*2d Sept.* Pulse 100, temp. 100°; clear percussion note all over lower part of abdomen. He is much more jaundiced, and is slightly delirious.

*3d Sept.* Pulse 102, temp. 100·4°. Has been very delirious during the night, having risen from his bed and removed the catheter. *Vespere.*—Delirium not quite so violent as during last night, but it has continued more or less all day. He is now intensely jaundiced and his abdomen very tympanitic. On testing his urine, which still contains a trace of blood, the bile colouring matter is found in large amount, and the sugar and sulphuric-acid test gave a reaction which indicated the presence of bile acids.[1] Russell and West's[2] process yielded nearly 380 grains of urea for the twenty-four hours. This, upon the diet the man had, rather surprised me, but no doubt the fever process, and the restlessness associated with his delirium, had somewhat to do with its seemingly large amount.

*4th Sept.* Pulse 120, temp. 101·2°; was so delirious during last night that he could with difficulty be kept in bed. *Vespere.*—Pulse 130, temp. 102·6°. Opium steadily continued.

*6th Sept.* Since last report he has been rather better. To-day he withdrew the catheter, and, during an effort at defecation, passed nearly a pint of clear urine. The instrument was, however, reintroduced.

*14th Sept.* It is noted that on the 7th he became worse. The pulse and temperature, both of which had fallen, again rose, and his delirium returned. For the last two nights he has been with difficulty kept in bed. During the day he is quiet and rational. His pulse is under 100, his temperature ranges between 99° in the morning and 102° at night. The restless delirium, the wavy temperature, and the tympanitic condition of the abdomen, associated with, in its lower regions, a doughy feeling and obscure tactile impulse, make me suspect the presence of pus; but the aspirator, when used, withdrew air only. Its withdrawal, however, somewhat relieved him. Diarrhœa troubles him much, and resists all treatment.

*22d Sept.* Since the report ten days ago he has slowly improved; the tympanitis has become less, the diarrhœa has ceased, and he is now able to pass his urine, which is no longer ammoniacal, freely and easily. His temperature is seldom over 101°, and his general condition is better. Over the lower part of the abdomen there is still perceptible dulness on percussion, and the doughy feeling on palpitation is very evident, both signs, I imagine, of peritoneal matting and thickening.

*1st Oct.* The patient, as regards his most serious illness, is now quite well. The fracture of his arm—bad, to commence with—had scant justice done it during the days of his delirium, and is now quite ununited. The tendency to riding of the fragments has been overcome by means of extension, and a fixed apparatus has been applied with the hope that, to his rapidly returning bodily vigour, the process of bone-union may bear evidence.

Injury to the urinary bladder may occur in several ways. It may be, and most frequently is, occasioned by direct violence applied over the region of the viscus at a time when, distended with urine, it has risen above the protecting walls of the pelvic bones. In such cases there may be little or no evidence externally of the injury. Sometimes it complicates fracture of the pelvis, either as another result of the force which produced the fracture, or from laceration of its coats by some spicula of bone. Penetrating wounds of the abdomen may implicate it, or it may even be ruptured by excessive accumulation of urine in its cavity. In any circumstances it is a lesion attended with great and immediate danger, and followed by a train of symptoms marked and unmistakable. In the history of the cases detailed, these are pretty well shown—sudden acute pain in the abdomen, commencing within short time of the receipt of the injury, and quickly followed by incessant and futile attempts at micturition. Tenesmus, more or less marked, sometimes supervenes, adding much to the sufferings of the patient. Catheterism, practised it may be hours after the bladder was last emptied, is followed by little result as regards the amount of urine removed, unless, as no doubt has happened, the instrument finds its way through the rent directly into the cavity of the peritoneum. Then, indeed, several pints of urine may be withdrawn.

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[1] Harley on Jaundice, p. 61.

[2] Roberts on Urinary and Renal Diseases, p. 107.

The urine, too, is more or less tinged with blood: this in itself is an important indication, as, with the exception of cases of cystitis or malignant disease, hæmorrhage from the bladder is generally the result of laceration of its coats. Vomiting, abdominal tenderness, and tympanitis appear in rapid succession, signs of what too frequently proves a fatal peritonitis.

One additional symptom has in some such cases been noted, and in Bland's it was present—the occurrence of jaundice. This, if the investigators of the chemistry of the urine read their results aright, must have been the result of obstruction in the bile ducts. That it was not caused by direct injury of the liver we know; the way in which the man met his accident, and the fact that he had no pain or mark in the hepatic region, prove this. His urine, when examined, gave evidence of the presence of the bile acids; and, by Russell and West's method, that there was no falling off in the formation of urea, was conclusively shown. These two facts, if the suppositions of Budd[1] and Harley[2] be correct, go to prove that there was no fault in the liver itself. The most likely explanation, I imagine, is that obstruction was caused by spasmodic contraction of the bile duct, either as a result of the peritonitis, or from reflex irritation due to injury in the course of the genito-urinary tract. That its occurrence added to the danger of the case I hardly believe. Bile, *per se*, is not, we know now, the blood-poison it was once supposed to be.

Before leaving the consideration of the symptoms, I wish to refer briefly to one point of some moment in the history of such cases;

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[1] Budd, Diseases of the Liver, 467.

[2] Harley, Jaundice, its Pathology and Treatment.

it is the length of time that may elapse between the accident and the development of such symptoms as render the patient incapable of moving about.

In Case II. nearly *twenty-four* hours had passed before he came under observation, and then, though no doubt suffering most severely, he was able to walk with considerable freedom; while in another case which I saw some months ago, the man presented himself at the hospital *thirty-six* hours after his fall, walked into the out patient room, and from thence to the ward, making all the while little complaint of pain. He died five days afterwards, and post-mortem examination verified the diagnosis, and revealed an extensive rupture of his bladder.

It is in a medico-legal point that this observation has its strongest bearing, for it is just possible a defence might be founded on the fact that it was highly improbable that a person having sustained such a serious injury should not be at once rendered helpless by it.

With reference to *Diagnosis.*—The history and the symptoms, even in those cases where there is no external injury, are generally quite characteristic. Certain enough, at all events, they are for the treatment hitherto followed, a treatment which, in most of its details, has this wide applicability, that it is suitable in almost all cases of abdominal lesion. Should, however, operative interference be decided upon, an accurate diagnosis is of greater moment. Following the cautious use of the catheter, a proceeding which may be rendered of even more service if we combine examination by the rectum with it, physical examination of the abdomen should be carefully made. In most cases absolute dulness will be found to exist in the hypogastric, and possibly in one or both iliac regions; while, by palpitation, the presence of fluctuation may even be detected. The use of the aspirator will place the presence of fluid beyond all doubt, and a very simple examination of that fluid by boiling will determine, by means of its *smell*, the existence of urinary solids.

The *Prognosis* must always be a grave one; surgical records tell but a sad tale as to the issue in cases of injured bladder. Let me quote from the writings of some who have devoted attention to the subject:—

Mr Birkett[1] says that notes "of 50 examples of this injury show that, with the exception of *three* cases, all terminated fatally. Of the three cases which recovered in *one* only the symptoms were those of extravasation into the peritoneum."

Dr Stephen Smith,[2] of New York, reports five cases of recovery out of seventy-eight.

Mr Le Gros Clark writes, "I have never met with, or, perhaps I should say more correctly, I have never verified, an instance of recovery after rupture of the bladder,"[1] yet he does not believe that the lesion is necessarily a fatal one—in fact, in one case which he relates there can be little doubt that the bladder was ruptured, the only, and to him the greatest, reason for thinking that it was not, was, that the patient recovered.

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[1] Holme, System of Surgery, vol. ii. p. 481.

[2] Bryant, Surgery, p. 305.

Sufficient such testimony is to the great danger attending the accident, and now as to its *Treatment*. On this point there is, I believe, room for innovation.

Some cases dying rapidly, no doubt, in great measure from the shock attending the sudden outpouring of urine into the cavity of the peritoneum, admit of little help; their fate is too quickly sealed. But these are the exceptions, the majority living days, it may be many days, after the accident, die either from peritonitis or from the septicæmia which sometimes follows it. Dependent upon the situation of the rupture is the method of treatment pursued. In cases in which the viscus gives way anteriorly, and below the line of reflection of the peritoneum, the procedure necessary in all cases of urinary infiltration must be carried out. Free and deep incisions must be made into the infiltrated tissues of the abdominal wall, and followed by the application of poultices sprinkled with charcoal.

This form of extravasation is rare; much more frequently the rent is in the posterior aspect of the bladder, involving all its coats, and consequently opening into the great sac of the peritoneum. How seriously this membrane resents irritation, the sequel soon shows; acute inflammation makes its appearance—one step, however, it is, and a most important one, towards a cure; lymph is effused in and around the neighbourhood of the wound, and sometimes the pelvic peritoneum is shut off from that of the general cavity of the abdomen. This fortunate result does not often take place, and the explanation is an easy one; the form of peritonitis induced by infiltration of urine is not often sthenic, and attended with the formation of lymph; it is much more often that low form of the disease in which a sero-purulent effusion, having few organizable qualities, is the result. The nature of the accident, and its consequences, demand in the treatment two distinct proceedings: the first, to endeavour to keep the bladder empty and at rest, with the hope that the wound may heal; the second, to combat the attendant inflammation.

In order to carry out the first, the use of the catheter is necessary. This can be managed in one of two ways; either the instrument must be introduced at regular intervals, or it must be retained in the bladder. Attendant on its frequent use is the obvious risk that its point might, coming in contact with the injured surface, break down the tender adhesion which had been formed, and so re-establish the rent. Against its retention can be urged the mechanical irritation the instrument is apt to give rise to, owing to the body of the organ being always contracted around it. The use of the soft French instrument, however, quite overcomes this; and that it is a better and safer proceeding than passing an instrument frequently, I have no doubt.

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[1] Diagnosis of Visceral Lesions, p. 332.

As regards the peritonitis, here, as in all forms of this disease, we can most rely upon the use of opium. Over and above its quieting and curative influence in such cases, it has the additional advantage that it diminishes very appreciably the amount of urine secreted.

From the constant application of anodyne fomentations great benefit is derived, and in the free use of ice and weak beeftea we not only do enough to support the patient, but do the best to render the urine as bland and unirritating as possible.

So far we have considered what may be quite regarded as the ordinary course of procedure in cases of rupture of the bladder, and that it is not altogether unattended by success, the result in Dr Thom's case shows. The observations of those, however, who have had a large and extended experience, some of which I have already given, go to prove that the recoveries are very rare, the fatality attending the accident very great. With such records, written as it were against us, it behoves us to inquire whether there be not other means of treatment more likely to be followed by success.

One or two suggestions have been made,—that they have ever been successfully acted upon I do not know, but it is well to consider them.

Professor Miller says,—"Should the patient survive for a day or two, it is possible—as dissection has shown—that by inflammatory agglutination of the abdominal contents the great cavity of the abdominal peritoneum may be shut off from that of the pelvis, the latter becoming coated with lymph like an abscess, and the urine confined there; under such circumstances it has been proposed to tap the cavity from the rectum by a long and curved trocar."[1]

That this would be neither an easy nor a satisfactory procedure, my experience in the case I have related showed me. Even with great part of my hand in the rectum, it was with difficulty I could reach the upper border of the contracted bladder, and I certainly quite failed to detect such evident fluctuation as would have warranted the plunging in of a trocar.

Moreover, were this only to be undertaken after days had passed, and after peritonitis had been followed by such marked effects, the results one would hardly expect to be very satisfactory.

Mr Bryant,[2] after mentioning the method by retention of a catheter, writes,—"It seems probable that a free incision through the perineum into the organ, as in lithotomy, is the better practice." This proposal, with certain evident advantages, has, I think, less

[1] Practice of Surgery, p. 470.

[2] Practice of Surgery, p. 306.

weight when we can use so well and so safely the *soft* catheter. True, the outlet for urine through it is not so free or so direct as a full-sized lithotomy tube would make it; the question would very much depend upon the balance found when the increased risk attending such an operation was carefully considered with the amount of likely advantage to be gained from it.

Mr Holmes says,—"It has occurred to me whether it would be justifiable when the laceration clearly involves the peritoneal cavity, . . . to cut freely into the abdomen, draw up the bladder, unite the wounds in it with silver or carbolized-gut sutures, wash out the peritoneum and close the opening, leaving a catheter in the bladder to obviate any distension of the viscus which might reopen the wound."[1]

These words I read while my own case was getting well, but they became of more interest to me, and possibly carried more weight, inasmuch as the suggestion they convey is exactly what we had proposed to do had the aspirator failed. And that the suggestion is a valuable and practical one, and one well worthy of all consideration, I shall endeavour to show. That within the last decade abdominal surgery has made rapid and brilliant progress, all surgeons now acknowledge. To the anxious labour of ovariotomists is this in great measure due, and it is on the lessons that their experience has taught, that the advocacy of the treatment of rupture of the bladder by abdominal section can best be based.

One special class of case, that of rupture of an ovarian cyst, followed by peritonitis, bears, I think, no inapt comparison to a case of ruptured bladder.

In both we have a sac, containing fluid more or less irritating in its character, suddenly ruptured, and the contents effused into the cavity of the peritoneum. In both a similar result follows, general peritonitis of a low type, in many instances accompanied with more or less sero-purulent effusion, so that with the symptoms attending the progress of a most grave serous inflammation we are apt to have superadded the peculiar dangers of septicæmia. The subject of ovarian disease who had suffered rupture of the cyst, and in whose case the conditions I have referred to had supervened, was, until very recently, regarded as beyond the reach of all beneficial interference, but not so now.

Dr Thomas Keith,[2] writing on "Suppurating Ovarian Cyst," tells most graphically his experience; and to show how desperate his cases were, and how admirably well they did, I borrow somewhat from him.

In his first case, where suppuration of the cyst had occurred, and peritonitis as a consequence, he writes,—"This was probably the first time that surgery broke in upon an acutely inflamed peritoneum. The intense lividity, amounting almost to blackness, of the abdominal contents, and the spongy tenderness of inflamed intestine, were then strange to me, though little thought of now. Recent lymph was present everywhere, adherent bowel and mesentery hedged in the thick-walled cyst, the base of which was in a complete state of slough. Inflammation had gone on to gangrene, and there was intense putridity. . . . The operation lasted two hours . . . recovery without a drawback."

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[1] Principles and Practice of Surgery, p. 216.

[2] Edinburgh Medical Journal, *February* 1875.

And again in case vii., where pus had been removed by tapping, and the patient sent to the country to recruit previous to operation, the sac gave way. She was not seen for twenty-four hours after the accident; then "she was almost pulseless, quite cold, with lividity of hands and feet, while the laboured catching respiration showed the nearness of approaching death. The abdomen was enormously distended and resonant at the umbilicus. It was evident that the cyst had given way, and that its purulent contents had escaped into the cavity of the peritoneum. Though it seemed then almost useless, I turned her on her side, opened the abdomen with a lancet at the umbilicus, and pressed out two basinfuls of pus and fibrinous clots. . . . Pulse on the morning of operation was 110, very feeble, and temp. barely 97°. Adhesions were almost universal, mostly recent, and bleeding was unusually free. The peritoneum was of a livid colour, showing everywhere signs of recent inflammation. 26th day.—Patient is quite well."

Mr Spencer Wells,[1] in considering this question, makes this statement,—"In several of my cases of ovariotomy, the operation was performed after the cyst had burst, and its contents had escaped into the peritoneal cavity. The peritoneum has been found intensely red, thick, soft or villous, and occasionally covered by loosely adherent flakes of lymph. Yet the result has been surprisingly satisfactory. The irritating cause having been removed, the irritation has subsided. If the cause had not been removed, death must have happened at no distant period, as all the general and local symptoms of chronic diffuse peritonitis had in the whole series of cases followed the rupture. Twenty-four times has this complication presented itself out of the last three hundred of my operations. Five of the patients have died. . . . At any rate, the bursting of the cyst or the filling of the peritoneum by oozing from the puncture made by tapping the cyst is no bar to the operation of ovariotomy, but rather a reason for doing it without delay."

Such a statement as this, taken in connexion with the cases of Dr Keith, prove unquestionably that, at all events, inflammation of the peritoneum having supervened, should, instead of being a reason against, go strongly for abdominal section in cases of rupture of the bladder. The mere incision would be but the preliminary step, and were it productive of no other good, it would, at all events, enable the operator to sponge out thoroughly the inflamed cavity, removing at one and the same time what remained of the irritant cause of the inflammation, and the dangerous products to which the diseased action is so apt to give rise. That even this would be highly beneficial, the experience of ovariotomy shows. Fully one third of the deaths following this operation are the result of septicaemia, a direct issue of the absorbent power of the peritoneum. Lightly as this sensitive membrane must be dealt with, most operators acknowledge that the risks from irritation following its careful cleansing are small compared with those following the retention of decomposing fluids in its cavity. Wells[1] says,—"When I began to insist on the importance of this process, other operators said it was unnecessary or injurious; that ovarian fluid in the peritoneum was harmless; or that the time lost in removing it, or the irritation caused by the sponging, were greater evils then a little fluid or blood left in the cavity. . . . I have regretted incomplete sponging, never that I had been too careful."

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[1] Diseases of the Ovaries, p. 79.

Bland's case afforded evidence of how much good might be done in this way. It taught not only the rapidity with which effusion may follow the establishment of peritonitis, but the danger attendant upon its retention. Twenty-four hours after the primary aspiration, the urine ceased to flow by the catheter, and superpubic dulness indicated the presence of fluid in the peritoneum. Having every reason to think that the bladder had again given way, and that the urine was again being extravasated, Mr Spence tapped the peritoneum, and withdrew some ounces of bloody fluid. This on examination proved to be inflammatory effusion and not urine. It was sero-purulent, unorganizable, and was no doubt a potent septic fluid. Days after his accident, and when the gravity of many of his early symptoms seemed past, active delirium, wavy temperature, a feeble pulse, and typhoid look—all told the tale that what necessarily was left of this fluid, or what of it was afterwards poured out, had done its work. That his ultimate risks were as great from his septicaemia as from his peritonitis, we all believed.

Then, as to securing the wound in the bladder in most cases of intraperitoneal rupture, this could with ease, I think, be accomplished. Occuring, as the laceration generally does, behind the region of the urachus, an incision between the umbilicus and pubes would give a fairly good view of it, and the general laxity of the vesical connexions would enable its being readily drawn into the abdominal wound.

Once there, catgut sutures should be introduced, avoiding in their introduction the mucous membrane. The success of Professor Brun of Tubingen,[2] in treating by silk suture the bladder wound in the high operation for stone, gives proof of the safety of the proceeding.

Even after such an operation, attended with success in all its steps, the retention of a catheter in the bladder for at least a fortnight would be necessary. If thought fit, and it would I believe be a safe measure, a glass drainage-tube could be passed from the lower part of the abdominal incision to the bottom of the recto-vesical fold, so that primary adhesion of the laceration failing, and extravasation again occurring, the fluid could be removed.

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[1] Diseases of the Ovaries, p. 383.

[2] Dr Lutzbeck of Tubingen, Deutsche Klinik, No. 15. 1858.

One parting word as to the use of the aspirator. That it is a safe and valuable proceeding I do not doubt, and that it was productive of much and decided benefit in Bland's case I believe; still, after having thought often and anxiously over the matter, I have come to the conclusion, that taking it all and all, the treatment advocated by abdominal section is really the most likely to be followed by success.

## ARTICLE IV.—*On the Therapeutic Action of the Secale Cornutum.*

By JAMES CUTHILL, M.D., Assistant to the Extra Physicians at the Royal Hospital for Sick Children, Edinburgh.

THE well-known energetic influence of the ergot of rye upon the parturient uterus naturally led to the employment of such a potent drug in various other conditions of the system. It has accordingly been used most notably in the hæmorrhages generally in hæmoptysis, in hæmatemesis, in epistaxis, in purpura, etc. More recently, it has been tried in cases of atony of the intestines, accompanied, as such a condition is liable to be, with irregularity in the alvine discharges, and the consequent generation of gases—flatulence in the bowels.

With the view of observing the effect of ergot upon the contractility of the smaller vessels, I have on recent occasions several times put a frog's foot under the microscope, and after, by means of the micrometer, noting the natural size of the vessels, watched the effect of the subcutaneous injection of ergotine, and on each trial have observed that most markedly the arterial radicles were reduced in their calibre to the extent of about one half, with accompanying stasis of the blood current.

Following out the observation, and remarking that this contractility must necessarily result from the action of the secale upon the fibres of the involuntary or unstriped system of muscles,—an influence which cannot reasonably be confined to one particular region of the body,—and whether we regard such contraction as occurring through the medium of the blood, or by the circuit of the cerebro-spinal or sympathetic systems of nerves, we are equally confined to the deduction that its virtue must be general and not local, and will in like measure be equally developed in whatever quarter of the body organic muscular fibre is plentifully distributed, and that its effects should largely extend to the heart, to the stomach and intestines, to the bladder, to the uterus, to the spleen and other viscera whose anatomical structure to a considerable extent partakes of this substance.

Accordingly, in a heart case which lately presented itself, in which the prominent symptoms were palpitation, with some degree of valvular murmur and increased dulness over the cardiac region—in short, the ordinary signs of simple dilatation of that organ—liquor secalis cornuti was administered in 1 dr. doses three times a day, with marked benefit to the patient, and the gradual amelioration of all the symptoms. The success of such a case, of course, depended upon the absence of structural changes in the valves themselves, the pathological condition being want of tone in the cardiac muscular apparatus, or flaccid heart as it might be designated, valvular incompetency being caused by dilatation of the orifices occurring synchronously with enlargement of the organ, and not to changes in the size or capability of the valves, which resumed their natural functions when the organ and its orifices returned to their normal dimensions.

I have not as yet had many chances for making clinical observations of a reliable nature on the action of ergot on the other organs indicated, but hope in a future paper to do so when more extended opportunities present themselves.

In conclusion, I would beg to indicate that, were the effects of different drugs upon the elementary tissues—on muscle, striped and unstriped, on nerve tissue, cerebro-spinal and sympathetic, on areolar and fibrous tissue, and on the osseous and integumentary textures—correctly investigated, much simplicity would be imported into our knowledge of the action of remedies, and a veritable advance be effected on the progress of rational therapeutics.

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## ARTICLE V.—*Notes of Cases of Susceptibility of the Action of Medicines.* By ROBERT SMITH, F.R.C.S. Ed.

CASES occasionally fall under observation which exemplify peculiar actions of poisons on individuals. Some of these actions may be peculiar in kind, that is, the poison may cause symptoms utterly at variance with its ascertained usual effects; or an unusually small dose may produce the usual effects of the poison in an aggravated degree, so that the person taking it may exhibit the symptoms that would naturally result from an overdose of the drug. In whichever form the unexpected result shows itself, we may refer it generally to idiosyncrasy, as to a convenient term; although in the latter case susceptibility might more nearly describe that peculiarity of system which renders it more sensitive to the action of any one drug than generally obtains. In practice it is important to bear in mind that some persons are so peculiarly constituted, and are so susceptible of the influence of certain drugs, that a very minute dose of these may produce effects such as we should expect to proceed only from a very considerable overdose. It is also of interest as a question of medical jurisprudence, and for these reasons we proceed to put the following cases on record:—

A young gentleman, æt. 19 years, had been struck on the left eye by a cricket-ball two days before he was seen. When examined, the injured eye was inflamed and painful, and the pupil was much contracted, while that of the other eye was in its natural condition, moderately dilated. Amongst other treatment he was directed to put two drops of a solution containing, *inter alia*, half a grain of sulphate of atropia, within the lids of the injured eye at night. This he did, but induced by the relief afforded, he also washed the eyelids on their external surface with the pencil, using, as I discovered on seeing his bottle, about half a drachm of the solution. The eye becoming less painful, he slept soundly. But on awaking he felt confused; vision was indistinct. Everything appeared in a haze. There was frontal headache, and a metallic taste in the mouth, accompanied by a constant desire to spit. Thinking something was wrong, he came at once to have it seen to, experiencing on the way an indescribable tendency to roll from side to side, which was only restrained by the assistance of a friend.

The pupil of each eye was now enormously dilated. Haziness of vision still continued, accompanied now at intervals by objects appearing double. It was only on a second effort that he succeeded in lifting a small bottle from the table, his first attempt having carried his hand several inches wide of it. The metallic taste still persisted, but there was no ptyalism, though the desire to spit was frequent; on the contrary, the saliva was scanty, white, and thickened, and the upper portion of the pharynx was hot and dry. The pulse was small and irregular, and there was headache, though not so intense as before his walk.

The application was at once discontinued, cold affusion resorted to, and stimulants given. In the course of the day he could walk without assistance, but the haziness and double vision persisted till the following day. The dilatation of the pupils continued gradually to diminish during the four ensuing days, when all unpleasant symptoms had disappeared.

Now, he had used in all about thirty drops of the collyrium, which itself contained only half a grain of the alkaloid to the ounce of water—a quantity in which it is entirely soluble—and of this it is certain some must have been lost in the manipulation; yet, from the $\frac{1}{32}$ part of a grain of the drug resulted very decided symptoms of poisoning, although the application had been only an external one. But I have frequently observed that the conjunctival covering of the eye is a very active absorbent.

The other case was that of a lady to whom it was necessary to prescribe a sedative; half a grain of powdered opium was given in the form of a pill. This she took, with the effect of becoming drowsy, a state quickly succeeded by heavy breathing and stupor. In this condition she lay for some hours, not asleep, but in a dreamy state of unpleasant half-consciousness, awaking gradually to the sense of a severe headache. When seen some hours later, the headache continued, and the pupils were contracted; the countenance was pale, as was the exposed skin generally, and there was a tingling sensation in the skin; there was confusion of intellect, evidenced by the vacant look at what was going on around, as well as by the difficulty of comprehension, which was apparent. She afterwards said that, after becoming drowsy, the last thing distinctly remembered was a sense of pain and constriction about the chest, which made her breathe with difficulty; and she first became fully conscious on the application of cold water to the head and chest; that is, when she was already partially recovered from the influence of the narcotic

## ARTICLE VI.—*Scald of the Glottis.* By J. M. COTTERILL, M.B., Resident Surgeon Royal Infirmary, Edinburgh.

As two cases of severe scald of the glottis have lately come under my notice in the Royal Infirmary, and as the symptoms, though intense, were successfully treated without resort to tracheotomy, a short account of them may be of some interest to your readers.

CASE I.—Mary N., aged 4 years, admitted at 5 P.M., 30th October, to ward 16, Surgical, under Mr Annandale's care. The patient's mother stated that, five hours previous to admission, the child had drawn into its mouth the steam from a kettle of boiling water. On examination, the whole of the back of the mouth was found to be red and inflamed. The respirations were about 35 per minute and noisy. The child was at once put to bed, and kept in a warm and moist atmosphere by means of a steam inhaler placed under the bedclothes, which were formed into a canopy over the patient by means of a large iron cage. As in the course of two hours the respirations had risen in frequency to 45 per minute, and were more laboured and stertorous, fifteen drops of antimonial wine were administered in a little water, which caused the patient to vomit, from which she at once derived considerable relief. Nausea was kept up by five-minim doses of the same every two hours till about midnight, when she dropped asleep. Towards morning there was a considerable aggravation of the symptoms, the respirations being between 60 and 70, the face deeply congested, and the dyspnœa intense. Before resorting to tracheotomy, the antimonial wine was again tried, and with an equally satisfactory result as before, first in the full, and then in the smaller doses. This was continued till ten o'clock next morning, when the patient was much improved. She continued to mend rapidly from this time, and left the hospital, cured, on 5th November; the only remains of the injury being some slight discoloration of the mucous membrane at the posterior part of the mouth.

CASE 2.—Patrick F., aged 3 years, admitted to ward 16 at four o'clock P.M., on 20th November. The patient's mother stated that the child had that morning drunk some boiling tea from the spout of the teakettle, and that suddenly about midday its breathing had become impeded, and was growing rapidly worse. The mouth, neck, and hands of the patient were found to be slightly scalded; the respirations were hurried, laboured, and noisy. The warm atmosphere was again employed, and the patient was carefully watched. At about two A.M. the symptoms were so much aggravated that I was suddenly called, and found that the respirations had increased to between 55 and 60 per minute, and the child was apparently becoming rapidly suffocated. In this case the additional complication of scald of the œsophagus rendered it a matter of great difficulty to administer fluids by the mouth; however, with some trouble, the patient was induced to swallow about ten minims of antimonial wine in a little water.

This had the desired effect of causing vomiting, and gave the patient speedy relief from the dyspnœa; but as he continued restless, owing to the pain of the scalds, five grains of potassium bromide were administered, which gave the patient sleep till morning. The dyspnœa being still present in some degree during the next two days, small doses of the wine were given now and then, and the patient only allowed a small quantity of milk and lime-water as food. From this time he continued to improve, and left the hospital, cured, on the 29th of November.

## ARTICLE VII.—*Note on the Treatment of Pityriasis Versicolor.* By JAMES RITCHIE, M.B., F.R.C.S.

RATHER more than three years ago I noticed, that whereas I could readily preserve specimens of the *achorion schœnleini* and also of the *trichophyton tonsurans* in glycerine jelly, satisfactory specimens of the *microsporon furfur* could not be preserved in the same manner. Although on treating scales scraped from patches of pityriasis versicolor with caustic potash, abundances of the characteristic bunches of spores were seen, the addition of glycerine jelly to another specimen from the same patch was followed by the disappearance, or at least by a marked shrivelling up, of most of the spores of the fungus, even of those which were lodged in the midst of masses of epithelium. It seemed probable that the acetic acid in the jelly was the active agent. That such was the case was shown by repeating the experiment with the acid alone.

If, therefore, acetic acid destroyed the spores, it might reasonably be expected to cure the affection (if it be caused by the parasite). I determined to try it in the first case which presented itself, and have since then used it constantly, and found it successful, even in cases in which other remedies had failed; *e.g.* one in which solution of perchloride of mercury gave only temporary relief, and another in which I tried the experiment of blistering a patch.

The method adopted has been to order that the skin be washed with soap-and-water to free it from grease, and thereafter, that there be applied daily to the affected spots a lotion consisting of equal parts of acetic acid and glycerine, also that the clothes worn next the skin be dipped in vinegar and water in order to free them from any of the spores which might be lodging about them.

One of the methods ordinarily recommended, and among others by Tilbury Fox, is to sponge the skin with vinegar and water, then to apply a lotion containing the hyposulphite of soda. The vinegar, I suppose, with the view of liberating the acid. Is the vinegar not the parasiticide?

The non-identity of the parasites in favus, ringworm, and pityriasis versicolor has been abundantly proved by Dr M'Call Anderson, as well as by others, and among these notably by the late Professor Bennett. The microscopic characters are sufficiently marked to allow of their being readily discriminated. The above difference in the action of acetic acid lends further, although very slight, evidence in the same direction. Again, whereas some have held that the presence of these spores is accidental, and others that they are the results of degenerations of the elements of the skin itself, the fact that the application of an agent which destroys the spores is followed by the cure of the disease, is strong presumptive evidence in favour of the view that the fungus has a causal relation to the affection.

I have found acetic acid equally efficacious in the treatment of a case of onychomycosis in which the *microsporon furfur* was present. A year ago I was asked to see a child, æt. 5 years, on account of an affection of the nails. Several of them were grooved both longitudinally and transversely; they were brittle, as evidenced by splitting; they had yellow pits here and there, and round the root of the nails there was a quantity of debris. Microscopic examination of some scrapings showed abundance of the characteristic bunches of the spores of the *microsporon furfur*. As there is in general a close relation between the nails of children and the skin of their scalp, it was thought advisable in this case to examine the scalp also, the result being the discovery of a yellowish scurf which showed under the microscope the same fungus as the nails. Both were cured by the above-mentioned treatment.

## ARTICLE VIII.—*Case of Accidental Penetration of Abdomen (per Vaginam); with Remarks.* By A. E. M'RAE, M.D., Penicuik.

THE following case occurred in my practice about two years ago. I took notes of it at the time, and these supply me with the details as given hereafter. The accident is an extremely rare one, and as the nature of the injury was accurately revealed by a post-mortem examination, I have been induced by several professional friends to place the case on record, hoping thereby to elicit the knowledge of others in similar cases, and in time, by the collection of the records, to deduce a principle of treatment.

A. P., aged 21, was seen by me on 24th July 1873. On my arrival, about an hour after the accident, I was informed by her master that she had been working on the top of a hay-stack, and while descending came against the handle of a pitchfork which was standing upright against the stack, and that it had entered her body. She herself also reported, "that she felt it go up through her until it came to her heart." She indicated the point of the sternum as the place where the handle stopped. She was then thrown to one side and fell on the ground; her master immediately came to her aid, and, being informed by her of the nature of the accident, he with very great difficulty withdrew the handle. He stated, also, that scarcely any blood came away.

Her mental faculties were quite intact, for I found her perfectly sensible and collected, though evidently in great pain. She seemed quite aware of the very serious nature of her injury, and exonerated her master from all blame.

The physical conditions were as follow:—Pulse 84, feeble, irregular, and intermittent; tongue very foul, and breath very offensive; she was very restless, and tossed about in great agony; her face was flushed, eyes suffused, and skin cold and clammy. On examination, a strong smell of faeces was observed on separating the thighs, but no faeces were observable on the linen or skin, nor had any been passed. A slight tear was seen on the right labium, extending inwards, and was jagged on the edges. Some fluid blood and small clots were issuing from the os externum. On making a digital examination, per vaginam, which gave her great pain, a fissure about an inch and a quarter in length, and running transversely, could be felt about an inch and a half from the os externum in the recto-vaginal wall. Through this fissure the finger passed readily into the rectum, and on being withdrawn was tinged with, and smelt of faeces. The uterus was *in situ*, and, with the exception of this fissure, the vaginal walls were normal. No other abnormality could be elicited in this region, but, externally, from the symphysis pubes to the ensiform cartilage, and more to the right than to the left, extended a band of tenderness about three inches in breadth. There was no discoloration or undue distension, nor was there otherwise any symptom denoting internal hæmorrhage. Thus far,—the rent in the labium,—the fissure in the recto-vaginal wall,—and the band of tenderness, corresponded with her own feelings that the handle went up to her heart.

In passing, I may mention that, while separating the labia for examination, a small pellucid transparent globule, about the size of a French bean, and having a comet looking opaque streak running through its substance, fell out on her linen. It had a small peduncle or cord attached to one end, and altogether looked very much like a two-weeks-old embryo. On putting it to her, however, she denied the possibility of being in the family-way, and as a fact likely to prove her assertion, told me that she never had menstruated, but at regular periods had intense headaches and flushings. Afterwards her mistress informed me, on inquiry, that for several weeks she had to be much on her guard to prevent the girl ruining her moral character; still this fact, without certain proof that the globule was an embryo, is too scanty evidence to prove that a non-menstruating uterus was capable of conception; unfortunately, before I got home in the evening, the globule had shrunk beyond all possible recognition.

On making out the serious nature of the case, I had her removed to the Penicuik Cabin Hospital,—a distance of nearly three miles,—and although carried on a "stretcher," the least jogging gave her intense agony.

At 9.30 P.M., after being put to bed, she felt shivery but not cold: pulse 88, temperature normal. I ordered an opium pill, and had the parts well washed antiseptically. At 11.15 P.M. I found her asleep; breathing, 24, easy; pulse, 100; temperature, slightly increased; skin, hot and dry; face, flushed and swollen; bladder, empty; had passed neither fæces nor urine; there was a strong smell of fæces about the body. While making this examination I found that she was chicken-breasted; the ribs as well as the sternum were very prominent across by the nipples. Her mother afterwards told me that, when pregnant with her, she had been kicked by a horse on the breast. One would think that the maternal impression of such an accident would have produced an opposite condition than that described.

It was impossible at this stage to do more than guess at the probable course the handle had taken. The evidences were conclusive that the abdominal cavity had been penetrated traumatically by a very rude instrument. There was the labial tear, the fissure in the recto-vaginal wall, the band of tenderness, the woman's own feelings, and, further, the handle was covered with blood and particles of fæces for about 18 inches. There could be no doubt, therefore, that it had penetrated a great distance into the cavity—in fact, the whole length of the cavity had been invaded. The probable courses it had pursued were, that, having entered the rectum, it had continued its course along the colon, making the colon so far accommodate itself to its unyielding structure; and the body being still in projectile motion, the force was not spent until the end of the handle had reached the under surface of the diaphragm; the colon being able to accommodate itself without rupture to its walls, but with probably great interference with its attachments; or—and I thought this latter the most feasible—the handle had passed through the rectal wall below the sigmoid flexure into the peritoneal cavity, being deflected anteriorly by the promontory of the sacrum, tearing its way through the intestines and their attachments, probably entering the lung. Either course seemed probable enough, and as either was a grave accident, only a most unfavourable prognosis could be pronounced.

On reviewing the case in my mind I found I was unable to come to any definite conclusion regarding the extent or exact nature of the injury, but that a large cavity lined by a serous membrane had been rudely entered by a foreign body, there could be no doubt, and that sooner or later peritonitis of aggravated type would set in. There was the fear that the bowels might be ruptured, and their contents escape into the cavity, and other vital organs, as the kidneys, liver, or lungs fatally injured. It was also possible that particles of straw—I found her clothes uninjured—might have passed in along with the handle; and as it would be impossible to remove them, their presence there, supposing the bowels, kidneys, and liver to be uninjured, would keep up peritonitis beyond the possibility of being subdued. With the possible injury to the kidneys, liver, and lungs, there was the additional danger of infiltration of urine, hepatic and fatal haemorrhage, haemoptysis and pneumonia, and pleurisy. With all these possible dangers in view, the treatment could only be at least for some hours palliative and expectant. As I have stated, there were no signs of internal haemorrhage, and though there was some cough, there was no expectoration of blood; I therefore inferred that there was no injury to the lungs or liver; so the indications were to soothe pain and keep the bowels from moving. For the former I ordered poultices and for the latter opium, and, by way of simple nourishment, a tablespoonful of milk and lime-water every half-hour if awake.

*25th July.*—9 A.M.—Face, flushed; tongue, very foul; breath, very offensive; temp. in mouth 99·4°; in vagina, 100·1°; pulse, 104, regular and full; abdomen, slightly hoven, and very tender between the umbilicus and pubes; bowels not moved, passed about one quart of normal-looking urine. Same treatment to be continued.

At 12.45 P.M. Temp. same; pulse, 130; constant feeling of pain over abdomen, with severe exacerbations; treatment continued; opium, poultices, etc.

At 4 P.M. She has vomited much bile; temp. and pulse same as before; she had again passed normal urine. I now felt sure that the kidneys had also escaped.

At 10.40 P.M. Pulse, 130; temp. in mouth, 99·8°; in vagina, 101·1°; right labium somewhat swollen; no motion of bowels or bladder; tongue, rather cleaner; breath, not so offensive.

*26th July.*—At 9 A.M., patient had passed a fair night; pulse, 110; temp. in mouth, 99·6°; in vagina, 101·5°; tongue, cleaning. Continue opium and poultices; increase the quantity of milk.

At 4.30 P.M. Pulse, 110; temp. in mouth, 99·6°; in vagina, 101°; had passed water; bowels very tender. There is a distinct bulging below and to the right of the umbilicus.

At 10.15 P.M. Pulse, 110; temp. in mouth, 99·5°; in vagina, 101·6°; had passed no water, nor had her bowels moved; tongue, still cleaning; breath, not offensive; wants more milk.

*27th July.*—Pulse, 110; temp. in mouth, 99·6°; in vagina, 101°; lying on her side, passed a very good night; slept once for two hours uninterruptedly; urine contains a large quantity of mucus; tenderness less; bulging not less prominent; wound in labium looking unhealthy, and there is a very fœtid fæcal discharge from the vagina; the vagina was syringed freely with sol. permang. potassæ.

4.45 P.M. Pulse, 110; temp. in mouth, 99·3°; temp. in vagina, 101·2°; she expresses herself as feeling wonderfully well; feels hungry, and wants solid food, which of course I denied; continue treatment as formerly.

*28th July.*—At 9.30 A.M. Pulse, 110; temp. in mouth, 98·6°; temp. in vagina, 100°; passed a fair night; still wakes up occasionally and starts with pain and a feeling as of something tearing up her inside; wound in labium looking clean and healthy, showing fresh granulations; no fœtor from vagina; abdomen still tender, especially over the bulging; has a feeling of a beating throbbing pain over same spot, and she says *it feels like "boiling"*; tongue, moist and cleaner; skin, natural; passed water, normal.

At 3.30 P.M. Asleep; observed, on watching beside her for some time, a quivering of the muscles came over her face and passed down her arms, which were shaken spasmodically for a few seconds; then she awoke with great pain; in a few seconds the pain entirely passed away, and she expressed herself as well again and painless.

At 10.30 P.M. Pulse, 125; temp. 100° and 101·7°; skin, warm; has been very restless and uneasy, having been disturbed—against orders—by visitors; having got opium, is now inclined to rest; has tasted nothing but water for some hours.

*29th July.*—9 A.M. She has passed a restless night; pulse, 125; temp. 101·1° and 102·1°; abdomen tender only over the bulging; lies mostly on left side; breathes freely; wound healthy; no fæces in discharges, nor fæcal odour from vagina; quantity of milk to be increased and opium continued.

At 9 P.M. Pulse, 140; temp. 101·4° and 102·5°; breath very offensive; tongue, moist; lying on right side; has taken a good deal of milk; feels a twisting, tearing, throbbing pain, now and again, rousing her up in agony; bowels rumbling much, and she has the feeling that they are about to move; opium to be repeated; ordered not to be raised up should bowels move.

*30th July.*—9 P.M. Pulse, 125; temp. 99·6° and 101·5°; passed a more favourable night; no motion; abdomen slightly tympanitic; bulging not dull, neither has it any appearance of pointing; tongue moist; wound in recto-vaginal wall healed; quantity of milk to be increased and opium continued.

At 9.30 P.M. Pulse, 115; temp. 99·9° and 102·4°; has had a fair day; no motion; no discharge from vagina; twisting pain occasionally; feels disposed to sleep; opium omitted, unless required.

*31st July.*—At 9 A.M. Pulse, 112; temp. 99·6° and 101·5°; passed a restless night; bowels not opened; abdomen, slightly distended with flatus, tympanitic all over except at bulging; passed wind per anum for first time; wound in labium healthy, but not healing; ordered weak beeftea.

At 11.50 P.M. Pulse, 120; temp. 98·9° and 99·8°; has been much troubled with pain; abdomen, slightly tympanitic and slightly swollen; dulness over a triangular space, having its apex on the right side, two inches below the umbilicus, base extending from right infer. spin. proc. to left of symph. pubes. She is unable to take a deep breath. Tenderness of abdomen increased; has vomited a mouthful of bile once or twice during the evening.

*1st August.*—At 9.30 A.M. Pulse, 112; temp. 99·2° and 101·3°; tongue, moist and clean; has passed a restless night; bowels tympanitic and swollen; pain agonizing; to have a tablespoonful of castor-oil and twenty drops of laudanum.

At 7 P.M. She feels well; bowels not moved; has had an egg, beeftea, and bread, against orders.

*2d August.*—1 A.M. Pulse, 120; bowels opened very freely, causing much pain; stool very copious, but not unwholesome; no blood; feels much relieved. No tympanitis exists; the patient feels hungry. I ordered beeftea and 1 grain of opium. I gave strict orders that she was not to sit up, and not to satisfy her craving for food.

At 7 P.M. Patient passed a fair night till 5 A.M., when bilious vomiting commenced. Being thirsty, her mother had allowed her—though strictly forbidden—to drink copiously of cold water. She has a collapsed appearance, sunken eyes, and husky voice. The pulse is quite uncountable; temperature in the mouth, *nil*; in the vagina, 102·4°. The bowels are much distended and tympanitic, except over the triangular space mentioned above. She still vomits at times pure water. The body is lying supine, with the knees drawn up for the first time. I ordered a mustard poultice to be applied, followed by bran poultices soaked in laudanum.

At 9 A.M. She feels slightly relieved; the face is of a better colour; still vomiting at intervals pure water. I gave her 1 grain of opium and some warm milk-and-water; she soon after became quiet, and seemed asleep.

At 10 A.M. No improvement; she was seen by Mr Annandale, who agreed as to the existence of general peritonitis, caused either by extravasated fæcal matter, or the too free use of cold water. He also agreed with the policy of having given the castor-oil, the increasing distension becoming a source of danger in itself. He also agreed that this second and general attack of peritonitis had lately supervened from either of these causes, initiated by, but distinct from, the first traumatic and localized inflammation.

At 12 noon there was no change; pulse quite uncountable; temperature, *nil;* vomiting bilious matter.

At 2.15 P.M. She is pulseless, but expresses herself as feeling more easy.

At 2.25 P.M. She expired, calmly and quite consciously.

Next day a limited examination was with difficulty obtained.

The post-mortem examination took place 26 hours after death. Rigidity was not strongly marked. The body was well nourished, and seemed to be about 17 years of age. The bowels were greatly distended with flatus. On opening the peritoneal cavity, a large quantity of thin watery pus escaped, and we subsequently removed not less than 2 quarts, which welled up from every part of the bowels. The omentum was deeply injected in every part, and contained innumerable minute cysts of pus. The liver was healthy and uninjured. The small intestines were beginning to become matted together with lymph, quite recent, thin, and scanty. The peritoneum was deeply injected in every part. To the right of the sacral promontory, the peritoneum and sub-peritoneal tissues were lacerated, and showed no signs of healing. All the sub-peritoneal tissues of the pelvic cavity were injected, and contained pus, and possibly some particles of fæces. The smell of the pus in this cavity was excessively offensive—not so the more recently formed pus that we encountered on first opening the cavity. On passing the finger upwards beneath, or rather posterior to, the peritoneum, it discovered a solution of continuity of the sub-peritoneal tissues extending upwards behind the nephritic vessels, which were uninjured as far as the posterior edge of the liver.

This space may be described as a canal, extending the above described distance, with its walls approximating and terminating in a cul-de-sac. This canal contained a small quantity of old pus, thick and laudable, and several pieces of hay and straw 2 and 3 inches in length. On passing the finger downwards between the sacrum and the uterus, it penetrated another canal also terminating in a cul-de-sac, and containing decomposed pus and several pieces of hay and straw. I now passed the forefinger of the right hand per rectum to meet the forefinger of the left hand in the latter cul-de-sac. A very thin partition existed between, about three inches or so from the anus; this partition was with some difficulty broken down, being the recently healed wound in the rectum, where the handle had entered the cavity, and the fingers met. On removing the fingers the fluids of either cavity mingled with those of the other. The rupture in the recto-vaginal wall could not be made out, though vigorous attempts were made to break down the adhesions with one finger in the rectum and the other in the vagina. The bladder was empty, the womb uninjured, and about its normal size.

On making a vertical section of the uterus, the knife went through the fundus with some difficulty, especially so in the cavity, the cutting of which felt gritty. The cavity contained a small quantity of gray-looking mucus. The substance was quite bloodless, and contrasted strongly with the surrounding deeply-tinted tissues. It looked and felt more like a fibrous tumour. The right broad ligament was torn from its parietal attachments, and its substance also was torn and deeply injected. The ovaries were involved in the inflammatory condition, and their state could not be made out satisfactorily.

The adhesions of the intestines were quite recent, and easily broken down everywhere. The only parts that had healed were the two ruptures in the rectum. The walls of the small intestines had not been ruptured.

The right labium was torn perpendicularly, and the tear extended for half an inch into the fourchette. The walls of the stomach were quite healthy, as also those of the intestines, although the whole peritoneal lining was involved in recent and acute inflammation.

Externally the breasts were well developed; the areolæ were very dark for an inch round the nipples; the papillæ were enlarged and prominent.

*Remarks.*—From the above described conditions it will be seen that the handle of the fork, bearing several pieces of hay and straw, had, by enlarging the os externum, penetrated the vagina, entered the rectum by a rent in the recto-vaginal wall an inch and a half above the anus, again piercing the right wall of the rectum about one and a half or two inches above its point of entrance; thence through the utero-rectal pouch, carrying with it the particles of straw, and probably now some particles of fæcal matter, tearing the peritoneum and sub-tissues in front and to the right of the sacrum, being deflected from its course by the brim of the pelvis and sacral promontory; finally, running up behind the peritoneum and right nephritic vessels beyond the transverse arch of the colon. From the condition of the breasts and the presence of the embryo globule, combined with the suspicions of the mistress, I am of opinion that the girl was pregnant, although I doubt much, with the above described uterus, whether a fœtus would have gone on to its full time.

Regarding the forensic aspect of the case, I have not the slightest doubt that the injury was purely accidental. The nature of the accident, the estimation in which the girl was held by her employers for her industrious habits, and her own uncontradicted testimony, all go to prove that the occurrence was unpremeditated. Besides, as the girl might choose any point in the circumference of the stack for her descent, the chances were as 1 to 216—reckoning the stack 12 yards in circumference, and the handle of the fork 2 inches in diameter—that she would select the point where the fork stood for her descent, even supposing the master had placed it there with a felonious intent. The habit of placing forks against stacks while being built is common, but the dangers of such a practice should be made widely known.

As regards the treatment, I may say that I looked upon the case as utterly hopeless from the first, and the post-mortem conditions only too plainly bore out my suspicions. I kept her living till within a few hours of her death upon milk and water only, believing this dietary the most favourable to the healing processes, and being anxious to avoid the excessive development of lymph, so common in abdominal inflammations. The salvation of the case was also the death; for, the opening to the cavity being closed, the pus could not escape; and if the opening had not closed, the fæces would have escaped into the peritoneal cavity, and accelerated the fatal issue. The marvel is, that the fæces did not find their way into the cavity in quantity. Of course the presence of the pieces of hay and straw prevented the wounds from healing, and set up suppurative instead of plastic inflammation. More stimulating food would have set the bowels moving, and the opposite condition of rest was indicated. I do not see that any other treatment would have averted the fatal issue, although it might have been deferred had she not been allowed to drink so freely of cold water during the night preceding her death. Had I known at the time of the accident what I knew after the post mortem, I might have searched for another wound, and inserted a drainage-tube, but that would not have removed the particles of straw. The termination shows how much the cases of country surgeons are at the mercy of ignorant and improvised nurses. During the course of the illness, the thermometer registered as follows, viz.:—The morning markings in the mouth thus, 99·4°, 99·6°, 99·6°, 98·6°, 101·1°, 99·6°, 99·6°, 99·2°; the average being 99·5°; the highest, 101·1°, being registered on the 29th, five days after the accident; the lowest, 99·2°, on the morning preceding her death. The evening readings of thermometer were 98·0°, 99·8°, 99·5°, 99·3°, 100°, 101·4°, 99·9°, 98·9°; the average being 99·6°; the highest, 101·4°, occurring on the fifth night, and the lowest after fever set in, 98·9°, on the day preceding her death. The morning markings in the vagina thus—100·1°, 101·5°, 101·0°, 100°, 102·1°, 101·5°, 101·3°, 102·0°; the average being 101·2°; the highest being 102·5°, on the morning of the 29th. It then fell to 101·3°, and rose on the morning before her death to 102·0°. Its lowest point was 100°, on 28th July, on the day before it reached its highest. Night markings thus—101·1°, 101·6°, 101·2°, 101·7°, 102·5°, 102·4°, 99·8°; the average being 101·4°; the highest, 102·5°, on the night of the 29th; the lowest being 99·8°, on the night succeeding the highest. It will thus be seen that the highest temperature was reached on 29th July, five days after the accident, but it only once reached within 2½ deg. of what is considered as a dangerously high point in traumatic fever, viz., 104·9°.

Another fact of interest is, that some hours before her death the thermometer registered *nil*, when kept in the mouth for the usual time, although it registered an increase in the vagina. This may be accounted for by local causes, the greater quantity of blood being sent to the neighbourhood of the injury; ultimately, about two hours before death, it gave no markings in either locality. Again, as regards the pulse, it did not fail to give a fair indication of the seriousness of the girl's condition. I may give extended the state on the different days:—24th July, 84, 88, 108; and on 25th, 104, 130, 130, 130; on 26th, 110, 110, 110; on 27th, 110, 100; on 28th, 110, 125; on 29th, 125, 140; on 30th, 125, 115; on 31st, 112, 120; 1st Aug., 112, 120; on 2d, 120; and latterly uncountable.

It will thus be seen that the highest number of beats, 140 per minute, was registered on the night of the 29th, when the thermometer also registered its highest mark; and the lowest, 100, on the 27th, when the thermometer also registered a fall for the first time since the fever set in. It is also interesting to notice that the reactionary fever had set in by midnight, for the pulse rapidly rose from 84, ninety minutes after the accident, to 108, five hours later.

Next day it rose to 130, and remained for many hours at that point. Again, although the pulse rose rapidly on the first night, the thermometer did not register a deviation from the normal temperature until the following morning. It will thus be noticed, in looking over the figures given above, that this was observable throughout the course of the case, for even on the morning of 2d August, when the pulse indicated demise, the thermometer registered 102 in the vagina. This observation is of course consistent with the well-known fact that, subject to varying conditions, the body retains its heat for many hours after death.

# Part Second.

## REVIEWS.

*The Natural History and Relations of Pneumonia: A Clinical Study.* By OCTAVIUS STURGES, M.D., F.R.C.P. London: Smith, Elder, and Co.: 1876. Post 8vo, pp. 336.

EVERY now and then we hear expressed—*ex cathedra* or otherwise—the opinion that inflammatory disease will, ere long, change its type, and that pneumonia, in particular, will again become sthenic in character, and require for its treatment the copious bleedings which were at one time indulged in. It ought to be a sad blow to such expectations to find that the tendency of modern speculation—as represented by Sturges, Juergensen, and others—is to remove pneumonia altogether from the class of inflammatory diseases, and to place it among the specific fevers; more especially, as we learn from Murchison and others, that, though these specific fevers have—like pneumonia—been often treated by bloodletting, they have never required, have never benefited, and have often been harmed, by such treatment.

There is nothing more remarkable in the history of medicine than the thoroughly scientific method by which Cullen—to his own satisfaction at least—established bloodletting as the only correct treatment for pneumonia, except the manner in which his treatment has been clung to after his basis has been proved to be untenable, and every now and then attempted to be revived for reasons which he himself would undoubtedly have regarded as altogether insufficient. The earlier physicians were, as we know, unable to make any accurate diagnosis, physical or otherwise, of the condition of the internal organs; their treatment was therefore entirely symptomatic, though to some extent based upon pathological views of the most hypothetical character. About the end of the seventeenth and beginning of the eighteenth centuries, however, dissections began to be made of the bodies of those who had died of disease, and from the records of those dissections by Bonetus, Morgagnis, and Lieutaud, it was gradually ascertained that in all those who had previously exhibited the symptoms of pneumonia or pleuro-pneumonia, the lungs were either filled with a solid material of a more or less dark colour, or were in a state of suppuration. Cullen's practical mind seized hold of this fact at once; he connected it on the one hand with the hæmorrhage (rusty sputa) always occurring from the lungs in such cases, and on the other with Hoffman's spasm of the arterioles as the theoretical cause of fever and inflammation. And, following out this idea, he promulgated the theory that pneumonia was an acute spasm of the pulmonary arterioles, followed by intense pulmonary congestion, which, if not relieved, ended in rupture of the bloodvessels and coagulation of the blood within the air-cells—a condition of matters which, if not fatal in itself, always terminated in suppuration, which was very generally fatal, and recovery from which was always very uncertain. With so lucid a pathology it was impossible to err; bloodletting *usque ad deliquium* was the only remedy that held out any probable hope of cure; it was at once the most powerful resolver of spasm, and the most certain remover of congestion; and as in every fatal case of pneumonia the lungs were found—necessarily, as as we now know—either in a state of red or gray hepatization, death could always be pathologically shown—from his point of view—to have occurred either from the remedy not having been pushed far enough or from its not having been had recourse to early enough. The matter was perfectly evident, lay in a nut-shell, and could be irrefragably proved. Moreover, Cullen found no difficulty in bleeding every one he believed to be suffering from pneumonia, whatever the state of their pulse; the cases recorded in his clinical lectures are precisely such as we meet with now-a-days, yet he found no difficulty in bleeding every one of them. In one case, indeed, he omitted this, fancying the disease to be typhus, when the dissection revealed a pneumonia—as it often does now-a-days in similar cases; not for one instant does he hint at any change of type in the disease, but he simply expresses his great regret that he had not recognised the disease, and had not bled the patient, and had thus unwittingly deprived the poor man of what he believed to be his only chance of recovery. In his "First Lines," moreover, he tells us that even in his day there were many who fainted on the withdrawal of but a small quantity of blood; but he also tells us not to despair in such cases, because "they will always be found to bear after-bleedings better than the first, and this allows the second and subsequent bleeding to be larger, and to such a quantity as the symptoms of the disease may seem to require." And it so happens that we have his own authority in his own handwriting for the amount of blood, the loss of which so frequently caused fainting in the pneumonic patients of those days, when the disease and the patients were both so "sthenic" that "a pound of blood avoirdupois," ℥xx., was the hypothetical norm for a primary bloodletting for "an adult male of tolerable strength," and this was "iv℥" (sic). Are there any of our patients even in these degenerate days who would faint on the detraction of a smaller quantity? are there many even now who would faint on the detraction of so small a quantity? And yet in his own handwriting Cullen has stated that "this fact"—fainting on the detraction of only four ounces of blood—"and rule," that a second bleeding is better borne than a first, "*hold very universally.*" (*Vide* "First Lines," 2d edit., 1778, vol. i. p. 279, § 348, annotated copy in the Library of the College of Physicians, Edinburgh. Notes in Cullen's own handwriting.) Cullen himself thus summarily disposes of the question of the change of type in pneumonia, for we venture with confidence to assert that there is not one of our patients in the present day who could not bear the loss of at least four ounces of blood without any greater harm than a faint. And as the British soldier at Alma, Inkerman, or Balaclava was not less profuse of his blood than his compatriot prototype on earlier battlefields; as in surgery, the patients of Fergusson, Syme, and Lister have not been found less able to bear loss of blood than those of Guthrie and Cooper; so, too, in medicine, the British patient will, if treated with equal boldness, be found to part with his blood as readily now-a-days as in the days of Cullen, Gregory, and Alison, and we doubt not with precisely similar results. For information what these were, and whether it may be worth while to reproduce them, we may refer to the writings of Marshall Hall, and of Dr Bennett and the other controversialists of his day.

We must remember, also, that it is only the apparent truthfulness of the relation between Cullen's treatment and his pathology that has given to the former its pseudo-scientific character; and that the instant we deprive it of its pathological basis, his treatment becomes a mere empiric procedure, to be estimated solely by its results, and criticised with the same impartiality as we would the similar employment of the decillionth of a grain of phosphorus. But no one in the present day need be told that Cullen's pathology is baseless; that whatever shred of respectability the theory of spasm of the arterioles may yet retain in the pathology of inflammation, its significance is now known to be totally different from that accorded to it by Cullen; that the lung consolidation is not due to haemorrhage, but yet forms so necessary a part of pneumonia that even Sturges confesses that in its absence that disease would be unrecognisable, though he fancies it might exist; while what Cullen calls suppuration of the lung we now know to be but a mode of development of the exudation into the air-cells, which only proves fatal from failure of the organism to complete the necessary cycle of changes—a failure not likely to be remedied by previous depletion.

Throughout the history of physic as of physics "one increasing purpose runs," and the principle of continuity is as important in the one as in the other, though the mass of wood, hay, and stubble with which each succeeding generation of physicians have overlaid it has made it more difficult to trace in physic than in physics. In this onward progress, Cullen's place is not that he first recognised either red or gray hepatizations as essential to the existence of pneumonia—that is due to Morgagni—but that he built upon the pathology of his day such a pile of practical stubble that the blaze of its burning is still smouldering after the lapse of a century. And the scientific value of this stubble, which we fondly call practical medicine, is well shown by the fact that in all that time there is only one name to be opposed to Cullen's, that of Hahnemann, a quack, who sold *pneum*, and who to powerful bleedings and other active medications opposed the equally successful employment of infinitesimals. But for this opportune *reductio ad absurdum*, there is little doubt that at least a large part of the practitioners of the present day would be still bleeding their patients as vigorously as Cullen ever did.

This work of Sturges—to return to our text—is a most valuable and important one, from several points of view. As an example of accuracy of personal observation, combined with analytical power, it is second to no medical work of the present day, but it affords an almost equally apt illustration of the defective way in which medicine is still taught in our medical schools and universities. When a man like Sturges is left with so hazy a knowledge of medical history, that he quotes Wilson Fox as an authority for the fact that the crisis in pneumonia usually occurs between the third and eighth days of the disease, while Wilson Fox himself has to rely upon Ziemssen, Wunderlich, Thomas, and Bleuler for information as to a fact well known in old times, distinctly stated by Morgagni, and which ought therefore to be either stated as an accepted fact in pathology, or its history traced, and not merely referred to a recent authority as if it were wholly a modern discovery. Indeed, the leading idea of Sturges's book—that pneumonia is not a true inflammation, but is closely allied, if not identical with, the specific fevers—is not a novel doctrine at all, but was put forth very distinctly, about twenty years ago, by Wilhelm Ziemssen in the Prager Vierteljahrsschrift for 1858, and quoted in our own volume for the same year, at p. 384, etc. This theory was most carefully worked out by Ziemssen from the statistical point of view, and in this way he arrived at the conclusion that the curve of pneumonia did not coincide with the curves of inflammations generally, but corresponded rather with that of typhus—a name which was then employed generically to include all forms of continued fever having a typhoid tendency, and which was at that time regarded as representative of an entirely opposite, and to some extent antagonistic, morbific constitution. Since his time this theory has been adopted by several physicians, and by none more distinctly than by Juergensen and by our author. Unfortunately for Ziemssen's theory, twenty years ago typhus, typhoid, and relapsing fevers had not been differentiated, and each of them has seperate mortality curves—each, as it happens, quite different from that of pneumonia; unfortunately, also, he laboured under the disadvantage of having the mortality returns of only sixteen years to go upon, and that is far too short a time to afford anything like a satisfactory constant of the annual mortality. Quite recently, other observers, Mr Buchan and Dr Arthur Mitchell, in the Journal of the Scottish Meteorological Society, have investigated the same subject on the basis of thirty years' reports of the same mortality returns (those of London), and they have not found Ziemssen's views in any way supported.

On the contrary, they have found the mortality curve for pneumonia to be very steadily marked from year to year, and to be essentially the same as that from bronchitis which no one is likely to look upon as a specific fever, or to accuse of being anything but an inflammatory affection. The pneumonia curve also differs *in toto* from those of typhus, typhoid, or even simple continued fever, as well as from that of pleurisy, the curve of the latter being somewhat analogous to that of rheumatism and pericarditis. There is nothing, therefore, in these statistics—the most perfect in the world, and extending over a longer period than any others—that lends the slightest support to the theory that pneumonia is a specific fever of any kind, or indeed anything else than an inflammation pure and simple. True, it differs in many respects from the inflammations of other organs, but so do the lungs differ in structure from other organs. Pneumonia differs also in many respects from interstitial inflammation (cirrhosis) of the lungs, so does hepatitis from cirrhosis of the liver; but though we all acknowledge the latter to be the result of a chronic inflammatory process, no one yet has hazarded the supposition that the former is due to a specific fever. It is quite true that in pneumonia the fever often abruptly ceases long before the local lesion is resolved; in a healthy subject this, however, is unusual, as the resolution is in such patients very rapidly completed. But be that as it may, the rapid cessation of a frequently very high pyrexia with the continuance of the local lesion often for a very long time is a daily occurrence in embolic pneumonia, which is purely a local process, and could not possibly be supposed to have even the most remote connexion with any form of specific fever; and, indeed, it requires but a very slight acquaintance with pyrexial processes to discover that the period of convalescence or repair is never pyrexial, or, as Dr Burdon Sanderson has stated, "does not form part of the febrile process." However provoking it may be to find such illogical reasoning as to the pyrexia of pneumonia, it is if possible even still more annoying to find Andral quoted as an authority in hyperinosis, and the far more important and more recent observations of Virchow entirely ignored. In short, though full of accurate observations, and careful reasoning on the premises supplied by himself, this work of Sturges is yet so defective in regard to the actual history and true pathology of pneumonia, as to defeat its own end in the minds of all, except those of the profession who always accept the latest views propounded, if fairly presented to them on good paper and in good print; and to them it will do no harm—rather good—as it will make them hesitate to treat with activity what they have learned to look upon as a fever, while as an inflammation they would probably not have hesitated to attempt to cut it short. Sturges himself only wants a little more thoroughness in his knowledge of the history and philosophy of medicine to make a reputation deservedly second to none of the present day.

*The Principles and Practice of Veterinary Surgery.* By W. Williams, F.R.S. Ed., Principal and Professor of Veterinary Medicine and Surgery in the New Veterinary College, Edinburgh. Maclachlan and Stewart: Edinburgh: 1875: Second Edition: Pp. 740.

We cannot too highly commend this excellent work of Principal Williams, which successfully maintains and increases the high reputation of its author, and of the well-known Veterinary School to which he belongs. It would be as difficult to overestimate the importance of the subject of which it treats, as it would be not to recognise the graphic lucidity of the descriptions of disease, the rational character of their pathology, or the admirable character of the illustrations by which it is adorned, and its information enforced. The fact of its having so speedily reached a second edition is sufficient proof that our commendations of this work are not misplaced, and that the profession and the public—for it interests both classes—have as high an opinion of it as we ourselves have.

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*A Course of Practical Chemistry: Arranged for the Use of Medical Students.* By William Odling, M.B., F.R.S., Waynflete Professor of Chemistry in the University of Oxford, etc. Fifth Edition. London: Longmans, Green, and Co.: 1876.

This is a very excellent work on a very important subject, and one which we have much pleasure in highly recommending to medical students. The book has rapidly run through four editions, and this fifth edition not only maintains the excellency of the previous ones, but is even an improvement upon them. The book contains four chapters. The first is introductory, and treats of chemical reactions and chemical manipulation, and will amply repay a careful perusal by every student of medicine. The second chapter treats of Analytical Chemistry, and gives in a clear and scientific method the tests, the actions and reactions, and chemical properties of such bodies as are of the most importance, and of the most common occurrence. The third chapter treats of Toxicological Chemistry, showing the various tests for the different poisons, and the methods of using these tests. This chapter is specially commended to the perusal of students of medical jurisprudence. The fourth chapter treats of Animal Chemistry, including the composition of the tissues, normal urine, abnormal urine, urinary deposits, clinical examination of urine, urinary calculi, the blood, and miscellaneous animal products such as bile, milk, and bone. The fourth chapter is most useful to medical practitioners as well as to medical students. There is also an excellent index to the work.

We have much pleasure in highly recommending this work not only to students, but also to the general practitioner. We wish it a wide circulation.

*A Manual of the Diseases of the Eye.* By C. MACNAMARA, F.C.U. Third Edition. London: J. and A. Churchill: 1876. Pp. 614.

WE welcome a new edition of Mr Macnamara's Manual, the more so as we consider the present volume in many respects superior to the two editions that preceded it. We had occasion, in reviewing the first edition, to point out several points in which we considered it defective; and we are glad to observe that in this edition some of these defects have been remedied. Thus the author has, we find, given up the theory he formerly advocated—that the alteration in the curvature of the lens during the act of accommodation depended upon contractility inherent in its fibres; he has also modified the large doses of opium recommended in the first edition for certain painful affections of the eye; and he has admitted the benefit that may be derived from the administration of mercury in, at any rate, syphilitic cases of iritis. It is satisfactory to find an author that is open to conviction and profits by criticism; and we are therefore encouraged to indicate a few further points in the present volume which, we think, might with advantage be altered or modified in the hope that the fourth edition may be still more useful and perfect than the third.

At page 22 the author states that "the range of accommodation for vision at different distances varies in different individuals and at different periods of life; in the normal or emmetropic eye the nearest point of distinct vision is from three and a half to four inches, and the furthest point is at an infinite distance." In stating the nearest point of distinct vision in the emmetropic eye, we think the author should not have taken an average distance such as that he mentions (which pertains to a certain period of life—about the age of 25 years), but rather have said, "The nearest point of distinct vision must be within eight inches of the eye," etc., etc., in accordance with the views of Donders, which are generally accepted.

We notice that among the operations for trichiasis the operation of "repositio ciliorum," originally described by Celsus, and recently re-introduced into practice by Dr Snellen, has not been awarded a place. This we consider a pity, as that operation, in suitable cases, is undoubtedly of great service.

We have frequently inveighed against the use of a syringe for the purpose of applying a stream of water to cleanse the eyes of patients labouring under purulent ophthalmia. The danger of the use of such an instrument is twofold; it may, by pressure with the nozzle, induce rupture of a deeply-ulcerated cornea; or some of the virulent secretion may be carried by the water squirted from the syringe into the eyes of the operator or of an anxious spectator. The same objections apply to the use of that instrument for the application of a lotion to the eyes affected with purulent ophthalmia, which practice we regret to see approvingly referred to in the

Manual. Pieces of fine rag or pledgets of fine cotton-wool may with advantage replace the syringe.

We are somewhat astonished to find that the possibility of atropine developing a peculiar form of granular conjunctivitis in an uninflamed eye is spoken of with some degree of doubt. We have seen many instances in which this has occurred, where atropine has been ordered for the purpose of dilating the pupil so as to improve the sight in patients affected with incipient cataract involving only the central parts of the lens. The inflammation is certainly not identical with granular conjunctivitis, there being no follicular elevations, but resembles that affection in the circumstance that there are numerous papillary elevations of the conjunctiva. It is almost invariably associated with an eczematous condition of the skin of the lids.

In speaking of the treatment of conical cornea, the recent operative procedures recommended by Bader, Bowman, and Secondi have not been alluded to.

Mr Macnamara has, in this as well as in the second edition, divided iritis into the plastic, serous, and parenchymatous forms, instead of the ordinary English classification into rheumatic, syphilitic, sympathetic, etc. We do not think the change is for the better, and we especially object to the "serous" variety, which is identical in its nature with the disease described under the head of keratitis punctata. In both we have inflammation of the innermost layers of the cornea and of the iris, but when the inflammation more markedly affects the cornea it is termed keratitis punctata; when, on the other hand, the inflammation of the iris predominates, iritis serosa. We think both forms should be included under one term, such as kerato-iritis.

The chapter on Mydriasis and Myosis we think scarcely equal in merit to other parts of the work; we, in particular, miss all allusion to the characters of myosis due to spinal disease, whereby it can be differentiated from other forms of contracted pupil.

The use of prisms to ascertain the existence of binocular vision is thus described at page 535:—"The existence of binocular vision is ascertained in this way. If a prism be held with its base inwards before either eye, rays passing through it will be defracted towards its base, and, falling on the retina, will form an image internal to the macula lutea, which, being projected, as in Fig. 46, in the direction E (that is to the outer side of the field of vision), direct diplopia will be the result. But the other eye will immediately and unconsciously endeavour to correct this, and by an involuntary motion it will turn inwards (become inverted), so that the rays of light from the object under observation may fall upon a corresponding portion of its retina, internal to the macula lutea, and thus correct the diplopia." What really does occur, in such circumstances, is, that the eye in front of which the prism is held rotates a little outwards so as to allow the rays to fall upon its macula lutea, and thus permit of single vision; and the perception of the object is much clearer than if the image fell upon other corresponding parts of both retinæ. Besides, it must be remembered that the inner part of the one retina and the outer part of the other correspond, and not the inner parts of the two retinæ.

In the chapter on Disorders of Refraction and Accommodation of the Eye, we find no reference either to spasm or paralysis of accommodation, the latter an important and not very rare form of paralysis.

We have a couple of slight, possibly clerical, errors to direct the author's attention to. At page 30, the mirror of Liebreich's ophthalmoscope is said to be of four-inch focus. We have seen mirrors with such a focus, but they were inferior specimens, all the better instruments having a focus of six to nine inches.

At pages 276 and 277, the deposit formed by the action of the tears on an acetate of lead-wash is said to be the *carbonate*, instead of the *chloride*, of that metal.

The work has some fair chromo-lithographs and numerous woodcuts, of which some are good; but others (such, for example, as those illustrating the operations of abscission and for strabismus) are decidedly inferior, and might with advantage be expunged.

We have freely pointed out what we consider blemishes in a work that has much to commend it. We are by no means blind to its merits, which are such as to enable it to occupy a good place among the works on ophthalmic surgery. Its arrangement is excellent, and the marginal headings of infinite service. We would rejoice if other authors—especially such as are voluminous—would adopt that system.

As a rule, the descriptions of diseased conditions of the eye are careful and accurate, and the treatment recommended is usually that approved of by most ophthalmic surgeons of the present day. On the whole, therefore, we can recommend this as a useful and handy work on ophthalmic surgery.

*How to use the Ophthalmoscope: being Elementary Instruction in Ophthalmoscopy.* By EDGAR A. BROWNE. London: Trubner and Co.: 1876.

IN the small compass of 108 somewhat widely-printed pages (part of that space, too, occupied by illustrative woodcuts), Mr Browne has succeeded in imparting a large amount of information which cannot fail to prove of service to students or practitioners who are about to learn the use of the ophthalmoscope. We find instruction in the optical principles regulating the refraction and reflection of rays of light, with the actions of mirrors, prisms, and lenses on beams of light. The construction of the ophthalmoscope, and the appearance of the interior of the eye in health and in various diseased conditions, are carefully described. The information is given in a very concise, but we may also add in a very clear and forcible, manner.

In glancing over the pages of the little work we only discovered one error of any importance; this occurs in that part in which the method of determining the depth of an opacity in the media is described. The author states that "the pivot or turning-point around which the eye rotates coincides with the posterior pole of the lens, and in whatever direction the globe is moved, that point always remains stationary." Further on he says, "Therefore (in the erect image) opacities moving in the (*a*) same direction as the patient looks are in the anterior portion of lens, aqueous, or cornea, in the (*b*) opposite direction are in the vitreous, and the extent of movement is in proportion to their depth. (*c*) Those remaining stationary are situated at the posterior pole of the lens." A diagram is also given illustrating these points. As, however, the centre of motion is not situated at the posterior pole of the lens, but at a point a little posterior to the centre of the visual axis,[1] it therefore follows that opacities situated at the posterior pole of the lens are not stationary during the movements of the eye, but move in the same direction as opacities situated in the cornea would do, only the extent of movement is less. Opacities situated in the anterior part of the vitreous humour will in like manner move in the same direction as the eye is moved, while those deeply situated in that humour will be seen to move in the opposite direction.

The section of the work that treats of the appearances of disease in the interior of the eye, might with advantage be much extended, and yet not exceed the limits of an elementary guide. What is given appears to us clear and correct, the prominent features of diseased conditions being well demonstrated.

Many of the diagrams that illustrate the text are original and ingenious in their construction, and very instructive.

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## *Determination of the Refraction of the Eye by means of the Ophthalmoscope.* By EDWARD G. LORING, M.D. New York: William Wood and Co.: 1876: Pp. 61.

MUCH credit is due to Dr Loring for having been among the first to point out that the refraction of the eye might be determined *accurately* by means of the ophthalmoscope, and for having devised a form of this instrument whereby this could most conveniently be effected. In this brochure, the forms of ophthalmoscope best adapted for this purpose are described, as well as the methods whereby the degree of myopia, hypermetropia, or astigmatism can be estimated. To those who desire to employ the ophthalmoscope for these purposes, we can recommend no better guide than Dr Loring's pamphlet. But we feel obliged to express our conviction, that in the great majority of cases we can arrive at a more correct and satisfactory estimate of the refractive condition of an eye, and with not much greater expenditure of time, by the use of an ordinary case of test lenses. Some people, among whom we include ourselves, have a great difficulty in completely relaxing the power of accommodation, which is absolutely necessary to obtain satisfactory results by the ophthalmoscopic method; while, on the other hand, unless atropine be employed to paralyze the accommodation, the patient may not thoroughly relax *his* accommodation, and thus fallacy arise.

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[1] The position of the centre of motion varies slightly according to the refractive condition of the eye, being most placed furthest back in the hypermetropic eye, and most anteriorly in the myopic; but in none so far forward as even the centre of the visual axis (see Donders on the Anomalies of Accommodation and Refraction of the Eye, pp. 179-182).

It is generally imagined that the estimation of the degree of refractive error by means of the ophthalmoscope is specially serviceable in the case of children, who from youth, nervousness, or stupidity, are not capable of being tested by the more ordinary method. But what, may we ask, is the use of ordering spectacles for that class of patients? They would very soon be lost or broken, or the child, afraid of being the laughing-stock of his companions, would purposely destroy them. Practically, we should consider it advisable to wait till the child arrives at such a period of life as fits him for the ordinary method of examination before recommending the use of spectacles.

## *Traité des Maladies des Yeux.* Par le Docteur CH. ABADIE. Tome premier. Paris : Librairie Octave Doin : 1876 : Pp. 501.

As a rule, we object to notice any uncompleted work. We certainly think the fashion that generally rules in this country—so far at least as medical treatises are concerned—of not publishing a part of a work till the whole is completed, superior to the method that is too often adopted on the Continent of publishing a work in parts. Here we are very apt to associate these fragmentary productions with insinuating travellers whose mission is to induce unwary simpletons to subscribe to some work (described as of the greatest importance and of unquestionable merit) which is about to appear in parts, unlimited in number. The poor victims soon find that they must either patiently await the termination of the series, or sacrifice what they have already spent on one or more parts. These remarks, we are glad to say, do not apply to the volume before us. Even if the work were not completed, what has already been published would deserve a place on the shelves of all interested in ophthalmic surgery. In this volume, affections of the orbit, of the lachrymal organs, of the eyelids, of the conjunctiva of the cornea, of the sclerotic, of the iris and choroid, and of the lens, and the disease glaucoma which is difficult to locate, are fully considered. In French works we usually find a want of reference to the work done by British, German, or other foreign men of science. We are glad to observe that Dr Abadie has studied both the German and English literature of the subject, and not merely the French.

As specially worthy of commendation, we select the chapter on Affections of the Cornea, and especially the parts that treat of ulcerative Keratitis and Opacities of the Cornea, their treatment by tattooing, trephining, etc.

The work generally is tolerably complete, so far as it goes, and yet concise and practical; but we noticed two omissions we did not anticipate. There is no allusion whatever to the removal of the cataractic lens by suction, although a variety of methods of operating are described. We think this operation, as it is occasionally practised at the present day, deserves notice in such a treatise. We further observe that, although Quaglino's and de Wecker's methods of performing sclerotomy are described, and the efficacy of these operations in glaucoma considered, no reference is made to the operations of Hancock and Solomon, which are essentially incisions of the sclerotic, and which were advocated long before the operations of Quaglino and de Wecker were devised. No doubt the incisions practised by the English surgeons were made through the sclerotic encasing the vitreous humour, whereas the Italian and French oculists carry their incisions into the chamber of the aqueous humour; but, in our opinion, the action of the operations is identical; and even were it otherwise, we are of opinion that some reference should have been made to the operations of Hancock and Solomon.

We have, however, much pleasure in recommending this volume as a satisfactory work of reference on the subjects which are therein considered; and we look for the succeeding volume or volumes with the confident assurance that the completed work will not in any way fall short of the promise of merit shown by the first volume.

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*Histological Demonstrations.* By G. HARLEY, M.D., and G. T. BROWN, M.R.C.V.S. Second Edition: Longmans, Green, and Co.: 1876.

IT would be difficult for any accomplished histologist to understand why or for what class of students this book has been published. It pretends to be a guide for the student commencing histology, offering him a description of the methods he is to employ to make successful microscopic preparations, and at the same time to give short descriptions of what is seen. The book is illustrated by a number of woodcuts.

The microscope recommended to the student is complicated enough, and is said to be provided with "the additional advantage of a magnetic stage." Any one used to work with a microscope will know how *dis*advantageous such a piece of apparatus is. Short chapters are devoted to the description of the microscope and the mode of using it. A very remarkable direction is given for focussing when using a high power. "In using the high power, it should, as nearly as possible, touch the thin cover glass; then, *looking into* the eye piece, the tube is to be *raised* slowly until the object comes into view;" *i.e.*, exactly the opposite of what ought to be done: the tube should be *lowered* first by the coarse adjustment until the object is dimly brought into view, and then the fine adjustment ought to be used to bring the object clearly into view.

Nothing could be poorer or more indefinite than the technology of this book—witness, speaking of a carmine fluid, the student is told that "in this fluid the tissue may be soaked for twenty-four hours, dried, and mounted in Canada balsam." In speaking of nitrate of silver, we are told "it is useful for colouring delicate tubes, outlines of cells, and fine connective tissue. The specimens must be soaked from a few hours to two or three days," etc. These extracts will serve to show how indefinite are the notions of the author of this treatise on all the newer methods lately introduced into histology. No mention is made of the use of chloride of gold as an agent for demonstrating fine nerve fibrils, no description of the immense value of nitrate of silver is given, and the virtues of perosmic acid are quite unknown. Under the heading "brain and spinal cord," chromic acid is mentioned, and in the first paragraph the student is told to mount sections of a brain so hardened in glycerine, though afterwards the details of L. Clarke's method are given. With the value of Müller's fluid the authors seem quite unacquainted. In fact, drying and boiling are the chief methods indicated. As to any instructions for making sections, a razor and a Valentine's knife are recommended; but no mention is made of any of the various forms of microtome which have proved so serviceable.

In the descriptive part many inaccuracies occur, such as the following: In speaking of cells, it is said they are "uniformly constituted of a membrane termed the cell-wall, so arranged as to form a sac capable of enclosing both fluid and solid contents." Now, this view of the structure of a cell was long ago exploded, and we are sorry to notice that it is distinctly stated that "the hepatic cells are enclosed in a delicate membrane," when it is well known that they are devoid of such an envelope. Tesselated epithelium is said to occur in a single layer in ducts. In the instructions for examining the circulation in the web of the frog's foot, that animal is said to possess "claws." A very notable error is the assertion that hæmatine crystals are found in human blood. The author knows nothing of the fact that Debove has lately shown that basement membranes consist of a single layer of flattened epithelial cells. The figure of hyaline cartilage is simply miserable. In speaking of the development of bone, no mention is made of the importance of the "osteo-blasts" in this process. The description of the gastric follicles is very incomplete indeed. We are told that "capillaries are composed of a fine homogeneous membrane, with here and there oval nuclei." One of the most notable results of nitrate of silver has been to show that these vessels are in reality made up of epithelial cells joined by their margins. Caustic soda is still recommended as the best means for obtaining a view of the nerves of the cornea. Every tyro in histology knows of the value of chloride of gold in this connexion. The latter half of the book is devoted to morbid histology.

The extracts we have given may indicate how far short the book falls of the recent advances in histology; and the inaccuracies we have mentioned may serve to warn the student *not* to rely upon this book in the event of his having to undergo an examination by any one who knows anything about the subject. We refrain from mentioning many other defects, which will be apparent on reading it through.

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*The Practitioner's Handbook of Treatment, or the Principles of Therapeutics.* By J. MILNER FOTHERGILL, M.D.; Member of the Royal College of Physicians of London; Assistant Physician to the City of London Hospital for Diseases of the Chest, Victoria Park; Assistant Physician to the West London Hospital. London: Macmillan and Co.: 1876. Pp. 573.

IT is our honest conviction, after a careful perusal of this goodly octavo, that it represents a great amount of earnest thought and painstaking work, and is therefore one of those books which both deserve and are likely to survive. The author is evidently full of professional enthusiasm and energy, is ambitious of excellence as a practitioner, and is still at a great distance from that self-satisfied condition many attain to, in which, with ample professional employment and an average success in the use of remedies, they can disregard the scientific observations and discoveries of younger men, while contentedly plodding along the lines of a former generation. Dr Fothergill is far from despising or underrating the attainments of his predecessors, and indeed some may regard him almost as a *laudator temporis acti*, but he has a keen eye for modern therapeutics when they seem to be founded on ascertained facts in physiology and morbid anatomy, and to be in accordance with established principles of pathology. He is *au courant* with all that is new in the present day, and invariably endeavours to connect with such knowledge, in a logical manner, his expositions of the appropriate treatment; so that this book, although written ostensibly for the young and inexperienced, may be very profitably studied by those who have been practising their profession more or less empirically for thirty or forty years. Of course they are free to adopt or reject the views brought before them; but to consider and weigh them at all is a wholesome exercise of the mind, and new light may occasionally serve to illumine not a few enigmata of their previous experience.

We are disposed to think that our author places more confidence in therapeutics and exhibits a greater array of well-considered prescriptions now than will probably be the case ten or twenty years hence, when his head is gray; but we like his faith, and deem it vastly preferable to the cynical scepticism which some others profess—a scepticism profitable neither to themselves nor to their craft. The impression likely to be produced on calm-thinking, experienced, and skilled practitioners of the healing art, by these extravagant manifestations of contempt for the common faith in therapeutics, will be to raise doubts as to the competency of those who make them to dogmatize on points which they have inadequately studied; while, as to the uninstructed public at large, the effect will be, we fear, to shake their trust in educated men, and to hand them over, more submissive than ever, to the rapacity of quacks and impostors.

With regard to the plan of the work, we may say that it is in some respects original, and is successfully carried out. To use the author's own words, "It is not an imperfect practice of physic, but an attempt of original character to explain the *rationale* of our therapeutic measures. First, the physiology of each subject is given, then the pathology is reviewed so far as they bear on the treatment; next, the action of remedies is examined; after which their practical application in concrete prescriptions is furnished." We are informed that "for nine years the writer has been labouring with the definite design of producing the present work," and we are not surprised at the statement, because the undertaking was really a great one, and the labour demanded must have been very heavy in the midst of manifold professional engagements. The style is, on the whole, very commendable, and seems to improve as we proceed; but while many parts are carefully composed, we cannot help saying that others are rather slipshod, as if they had been hurriedly put together under pressure, and almost *currente calamo*. There are many obvious repetitions which the author himself confesses, and even justifies on the plea of their importance; but in a good book such as this, and one that deserves to become a professional classic, all avoidable redundancies ought to be removed. In preparing for a second edition, it will be important to keep this rigidly in view. The book will not suffer, it will gain. If it loses in dimensions, it will improve in unity of design, in compactness, in symmetry; and the reader will be more able to see its scope, and to understand the general bearing and significance of the principles laid down.

Another criticism may be permitted. The style occasionally becomes too familiar and colloquial for a book intended to take a permanent place in professional literature. We grant that it may be more lively in consequence for drowsy readers, but others will detect an incongruity with the subject-matter; and on the whole, we think that a strict adherence to clear, precise, accurate didactic statements would be preferable to the metaphorical language frequently employed; although we must concede that some of the forms of expression used in illustration are happy, and convey the author's meaning in a forcible manner. We venture to suggest, therefore, that in the prospect of a new edition of the book, some trustworthy friend, of severe, fastidious taste, should be invested with plenary powers for revising the proof-sheets. A fresh eye will often light upon manifest improvements, which an author will gladly acknowledge. The late Dr John Abercrombie, we have been told, was in the habit of submitting all his writings to a judicious professional friend of literary taste, prior to publication.

We shall now make a few quotations as specimens of the work. The chapter (IV.) on Body-heat and Fever will be perused with advantage by many who now use the pocket thermometer, without having very clear notions as to the significance of temperature and the lessons it conveys. Having described the typhoid condition in the graphic words of Dr Alexander Tweedie, he says: "With this condition all medical men of any experience are unfortunately but too familiar. It is a condition which, to a greater or less extent, obtains in every well-marked case of typhus, and is common in severe cases of enteric fever. From its frequent occurrence in specific fever it has acquired the name of the typhoid condition, and was once supposed to be found solely in specific fever. Consequently when this condition showed itself in the course of any other affection, the practitioners of old said, 'The disease has turned to typhus,' because they recognised a condition, as they thought, pathognomonic of typhus fever. In this they were distinctly mistaken. It is true that such a state is associated with a sustained high temperature—a condition most commonly supplied by specific fever—but is not causally associated with the specific fever *per se*, but with the sustained high temperature. They were then in error in supposing that the ailment had turned to typhus when they observed a typhoid condition coming on; but, ere the day of the clinical thermometer, the recognition of the association of this state with a persistent high temperature was simply impossible. Even yet the well-worn formula, that such a person's disease turned to typhus, which killed him, is heard from time to time. Still are patients suffering under different maladies sent into fever hospitals because they have passed into a typhoid condition." "The pathology of the typhoid condition is a matter so important that a section may well be devoted to its special consideration. Such examination of its pathology will both clear up its origin, and point the direction of the therapeutic agents to be employed. To commence with, it is necessary to point out this fact, viz. that at the ordinary temperature of the body nitrogenized tissues wear away but slowly, and their oxidation is a trivial affair. But as soon as the body temperature rises, the nitrogenized tissues begin to melt down by oxidation, and there is found a corresponding excess of hystolic products in the blood. The higher the temperature the greater the waste of the nitrogenized tissues, especially the muscles. After a fever, the fat of the body is left comparatively untouched, but the muscles are shrunken and wasted. A microscopic examination of the muscles after death from fever demonstrates that they are the subjects of extensive structural changes. . . . Such changes are not only found in the dead, but identical changes have been found in fever patients alive, and who have afterwards recovered. They are not, then, post-mortem changes; they are the changes wrought in the muscles by a sustained high temperature. They are not confined to febrile conditions of a specific nature; they are found also in the hearts of women who have died from puerperal septicæmia."

The following remarks upon the action of digitalis are important: "This drug has a notable history, and perhaps more than any other marks the passage of therapeutics from the regions of empiricism to the surer ground of the domains of rational medicine. Additional interest has gathered round it from the antagonistic opinions which have been and are still held by some as to its real action. In past days, when palpitation of the heart was held to be over-action of the organ, and hypertrophy was a disease to be subdued by active measures, digitalis was held to be a cardiac sedative, because it allayed the palpitation. It received the name of the 'opium of the heart.' Nothing can be more assured than the fact that palpitation may be usually subdued by its use. But we now know that palpitation is usually the outward visible sign of cardiac embarrassment, an active indication of debility indeed; and that hypertrophy is a conservative and compensatory growth, to be encouraged rather than otherwise. Consequently the old ideas of the way in which digitalis produced a quieter action of the heart have been abandoned, and truer views of its action have taken their place. So early as 1785, Dr Withering had observed, that as a diuretic it was of little avail in persons with a tight and cordy pulse. 'On the contrary, if the pulse be feeble or intermitting, the countenance pale, the lips livid, the skin cold, the belly swollen, soft, and fluctuating, or the anasarcous limbs readily pitting under the pressure of the finger, we may expect the diuretic effects to follow in a kindly manner.'

"Sir Henry Holland found that 'the enlarged and flaccid heart' was the condition in which digitalis was valuable. More recently digitalis has been made the subject of numerous observations and experiments at home and abroad. . . . When administered experimentally to animals, it is found to bring the heart to a stand-still in systole; and in my own experiments I have found that in the hearts of frogs paralyzed and flaccid in diastole from the effects of aconite, the administration of digitalis brought back the contractions, and, if the dose were large enough, produced the opposite condition of firm contraction in systole. About this time an opportunity occurred to my friend Dr W. Dobie of Keighley, of trying digitalis in a case of aconite poisoning with complete success. . . . . No doubt, however, exists now but that digitalis excites more perfect contraction of the muscular walls of the heart. The ventricular systole is more perfect, and the beats of the heart are reduced in frequency. 'The diminution in the frequency of the heart's action under digitalis always means an increase of the period of the dilatation of the ventricles. Pulse-traces readily show this.' (Balthazar Foster.) The consequence is that the heart's brief sleep is lengthened, and the addition thus given to its aggregate rest in twenty-four hours is considerable."

Another quotation on the same subject under a different aspect may be desirable:—"The blood escapes from the peripheral ends of the arterial system into the veins, and so returns to the right heart, by which it is pumped into the pulmonary circulation, where the blood is oxygenized; whence it once more finds its way into the left heart. It is at once obvious, that when the arteries are well filled with blood the veins will be less full; when the arteries are comparatively empty, the veins will be proportionately full. Venous fulness then indicates arterial anæmia, and our line of treatment, under such circumstances, is to fill the arteries, and so to empty the veins. In order to accomplish this, we administer an agent which will increase the vigour of the heart, while restraining the outflow from the arteries by diminishing the calibre of the arterioles: such agent we possess in digitalis. Consequently, when from any cardiac impairment the blood is insufficiently pumped out of the veins, and the arteries are unfilled and compressible, the administration of digitalis tends to restore the lost balance. It is obvious that this is not the agent to be chosen when we desire to lower the action of the heart, or to decrease the pressure within the arteries. Yet, from mistaken notions, digitalis is still used by some for the latter purpose. No wonder is there, then, that the use of so powerful an agent should be followed by disastrous results if it be given in the very cases where its use is contraindicated; or that the drug often gets a bad name, when really it is the knowledge of the administrator that is at fault."

These remarks on albuminuria seem worthy of being quoted (p. 396):—"Albuminuria is a valuable symptom when carefully appraised, but it will not settle the question of renal disease. It is a symptom not to be neglected, but it has been sorely abused; it has covered much culpable carelessness, and occasioned much mischief. The man who would make the diagnosis of chronic renal disease to turn on the presence or absence of albumen is a man whose patient I should not like to be. The silent progress of interstitial nephritis is often without albuminuria for long periods; the absence of albumen, therefore, does not contraindicate the presence of disease, even when far advanced; at other times, albuminuria is a temporary condition of no real importance. Albuminuria derives its significance from the conditions under which it occurs; and the great matter is to arrive at a knowledge of these conditions, after which only can we appraise the value of this sympton."

The chapter on the Nervous System (XXI.) will amply repay perusal. "Affections of the spinal cord are very difficult matters to approach from a therapeutic point of view. Inflammatory conditions must of course be treated in a manner similar to like affections of the intracranial contents. Conditions of congestion of the cord are to be treated by purgatives and by ergot, which exercises a decided effect upon the vascular system of the spinal cord producing contraction of the vessels. . . . . . In cases of anæmia of the spinal cord, with resultant paralysis, more or less absolutely free from spasms, either tonic or clonic, strychnia is indicated. It has been found to increase the vascularity of the cord, *dilating* the spinal bloodvessels, and, indeed, exercising an influence the opposite of ergot; and is accordingly to be given with iron, good food, and general tonics."

We particularly commend the chapters on Public and Private Hygiene (XXII.), on Food in Health and in Ill-health (XXIII.); and the conclusion—the Medical Man at the Bed-side. The last is high-toned, and indicates much shrewdness of observation. Our space will not admit of farther quotation. We content ourselves with again recommending the book very cordially, and venturing to prophesy that it bids fair for permanence, if the author will avail himself of the few friendly suggestions we have made.

DR MACLEOD'S CLINICAL NOTE-BOOK is the name on the back of a very neat case containing loose sheets of paper for notes, and fifteen pages of very sensible instruction and guidance in the art of surgical note-taking. The boards of it have, on their inner surface, a rough outline figure of a man, in front and back view; from which, on tracing-paper, copies can easily be made to illustrate the cases.

The work is published for the use of students, and sold at cost price, 2s. with, and 1s. without, the boards. No printer's name is affixed; but we learn it can be procured from James Haddon, 129 Sauchiehall Street, Glasgow.

*A Handbook of Therapeutics.* By SYDNEY RINGER, M.D., Profesor of Therapeutics in University College, etc., etc. Fifth Edition. London: H. K. Lewis: 1876.

HAVING expressed our high opinion of a former edition of this excellent work, it will not be necessary to say much on the present occasion. This fifth edition fully maintains the high reputation of the author—the very fact that a new edition is required almost every year is sufficient proof that the excellencies of this work are appreciated by the profession generally. We are acquainted with no work in the English language which gives so clearly and so fully the actions and uses of the various substances used in medicine. The work is excellently arranged, and the actions and uses of the various drugs are described in a manner at once interesting and instructive. The whole subject of therapeutics is treated by Dr Ringer in a masterly, scientific, and exhaustive manner. We have much pleasure in highly recommending Dr Ringer's "Handbook" alike to students, teachers, and general practitioners.

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*Recherches Historiques et Critiques sur l'Etiologie et la Prophylaxie de la Fièvre Typhoïde.* Par le Docteur NOEL GUENEAU DE MUSSY, Médecin de l'Hôtel Dieu, Membre de l'Académie de Médecine. In 8vo, pp. 127. Paris: 1877.

THIS short treatise of Dr Noël Gueneau de Mussy is a permanently valuable contribution to the literature of typhoid fever. It has, moreover, been published in Paris at a particularly seasonable moment, when a very severe epidemic of that disease is prevailing in the city, which its self-complacent inhabitants call "the centre of civilization," but which, nevertheless, in respect to some most essential sanitary requirements, lies outside the pale of modern science.

The author does not shrink from stating his views as to the inexcusably bad sewage arrangements of Paris. He says:—"Typhoid fever rages at this moment with unwonted severity. This was to be expected as a sequel to the long duration of intensely hot and exceedingly dry weather which we have experienced this year. Similar epidemics have been preceded by similar atmospheric conditions; and the influence of these conditions has been pointed out by nearly every author who has studied the disease. They are, however, only predisposing causes—auxiliaries to the direct cause which produces the fever. Are we impotent against this direct cause? Can we neither prevent nor even restrain its ravages? I believe that we do possess a controlling power. An extensive inquiry begun in France, and continued in England with as much sagacity as perseverance, has, I think, disclosed the secret of the origin of typhoid fever. We are at least acquainted with the *foci* in which it generally commences its existence; and it is to them that it behoves us to direct our efforts for the destruction of the germ. Very numerous facts of unquestionable significance demonstrate that sewage is the vehicle by which the disease is chiefly communicated. Sanitary measures, founded upon these etiological data, have been adopted with excellent results in many towns of Great Britain and Belgium. I am afraid that we are proceeding in an opposite direction in Paris. We pour the very imperfectly disinfected liquid portion of the sewage of privies into the sewers. We allow permanent communications to exist between privies and the sewers, which through large gratings freely exhale their putrid miasmata in the streets; they are so many open ways for the dissemination of the contagium of typhoid fever and the contagium of cholera, both of which are principally transmitted in the dejections. The object of the present work is to present to my medical brethren an abstract of researches made up to date into the etiology of typhoid fever, so as, through them, to call the attention of the administrative authorities to a question of the utmost importance to the public health."

The author gives an excellent account of the researches of William Budd, whose conclusions he adopts. He at the same time points out that whether the views of Budd or Murchison be adopted, the sanitary precautions which have to be taken are substantially the same; and that all such precautions are ignored in Paris. It is safer to act on Budd's theory, as it involves a careful disinfection of the dejecta of each patient, in addition to proper sewerage.

The present Parisian epidemic—now, happily, in its decline, both in respect to the number and severity of cases—has, like the epidemics of 1865 and 1872, specially selected particular districts, streets, and houses. It has, for example, particularly raged in the *quartier Saint-Laurent*, and in the great barracks of Saint-Eugène, where the malady became so general and so fatal that they had to be evacuated by the troops. Draw a plan of the sewers of Paris, and you draw a map of the topography of typhoid fever in the past and present epidemics. The great central sewer—the Cloaca Maxima of Paris—the length of which is 6329 metres, traverses the *quartier Saint-Laurent* from east to west, passes under the Saint-Eugène barracks, receives the Asnières sewer in the Rue de Château d'Eau, and proceeds onwards to the Champs Elysées, permeating, by house-drains and otherwise, streets and dwellings with its gaseous emanations charged with typhoid contagium. In some of the best old streets, where regularly emptied cesspools are still maintained, and where the houses do not communicate with the sewers, typhoid fever is always less prevalent.

The author, in an English dedication, inscribes his work to his old friend Dr A. P. Stewart in the following words:—"To my Friend Dr Alexander Stewart, late Physician to the Middlesex Hospital. My dear friend, thirty-six years ago we were both in Paris studying fever in Chomel's wards. You published then an admirable work which will attach your name to the history of this fever. You were the first, with all the strictness of the modern scientific method, to prove, as was asserted by very few and disputed by a great many, that typhus and typhoid fever were two diseases essentially different; and tracing every symptom of both those affections, you pointed out with undeniable evidence how dissimilar they were in their manifestations during life and in the organic changes after death. Allow me to dedicate to you, and through you to the English physicians who welcomed me with such gracious hospitality, this pamphlet, the principal and best part of which is borrowed from English medical works."

We have transferred this dedication to our pages with great satisfaction, as it gives Dr A. P. Stewart the high credit, to which he is unquestionably entitled, of being the first to demonstrate that "typhus" and "typhoid fever" are two distinct diseases. This he did in a memoir published in the *Edinburgh Medical and Surgical Journal* in 1840, which was reviewed in November of the same year in the *Archives Générales de Médecine*. The reviewer summed up his analytical notice by stating that Dr Stewart had demonstrated that in England there are two distinct fevers—*typhus* and *typhoid.*

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*Etude Expérimentale sur l'Entrée de l'Air dans les Veines; et les Gaz Intra-Vasculaires.* Par LOUIS COUTY, Docteur en Médecine, Aide-major stagiare du Val-de-Grace. In 8vo, pp. 172. Paris: 1875.

DR COUTY'S work consists of an introduction and three parts. The *first* part is given as an analysis of the labours of previous inquirers; the *second* is an account of the symptomatology; and the *third* is a statement of the author's conclusions as to the cause of danger and death from the entrance of air into the veins, and as to the treatment of cases of that accident.

Notwithstanding the author's imperfect acquaintance with the literature of his subject, and his useless resuscitation of some of the obsolete questions connected with it, his pamphlet is an interesting record of creditable work. It was not necessary to apologize for having undertaken the inquiry; but if any apology were needed, none could have been found more inadequate than that which is offered. "Not only," says the author, "is the explanation of the accidents caused by the entrance of air into the veins still much disputed, but the accidents themselves are studied in a confused manner, without order, and without continuity of observation." He adds—

"I have in vain searched for a clear and precise symptomatology in the various works on the subject." Dr Couty has, perhaps, confined his search to French books. He could not have penned the words now quoted if he had read Dr John Reid's masterly essay on *Death from Entrance of Air into the Veins*, contained in that author's "Physiological, Anatomical, and Pathological Researches," published at Edinburgh in 1849. In that critical digest of the literature of the subject, up to the date at which he wrote, John Reid lays down a clear and precise symptomatology, explains with exactitude the cause of death, and distinctly announces the treatment which he thinks most likely to be useful. In 1843 Professor Wattmann of Vienna published his monograph on the entrance of air into the veins [*Sicheres Heilverfahren bei dem schnell gefährlichen Lufteintritt in die Venen, und dessen gerichtsärtztliche Wichtigkeit*], in which he points out the causes and treatment of the accidents in question, and enters into full explanations of the discrepancies in the accounts of the different cases of the accidental entrance of air. Reid and Wattman quote, criticise, and in the main adopt the views of Cormack, referring to his Edinburgh Prize Thesis of 1837, and his pamphlet of March 1848, which, though specially controversial, was also a reiteration of the conclusions announced some months previously in the thesis, supported by telling references to the celebrated discussion then going on in the French Academy of Sciences, which commenced in December 1838. The works of Cormack, Watmann, and Reid, now referred to, are entirely ignored by Dr Couty. The names of Wattmann and Reid are not even mentioned; and though Cormack is named as an author holding certain opinions, and as having published a case observed by Syme, Willis, and himself, of death from air entering the circulation by the anterior jugular vein, no mention is made of his experimental inquiry of 1837. His place in the investigation is thus incorrectly described:—"Supported by Cormack in 1850, and afterwards in 1870, in a thesis containing nothing new except three surgical cases, in two of which the entrance of air was by the uterine veins—admitted also in part in the thesis of Meric, adopted by Longet—the mechanical cardiac theory was not really restored to its place of honour till 1873, by the important work of MM. Muron and Laborde." On referring to Cormack's Paris thesis of 1870, we find that the author quotes some of the experiments and all the conclusions of his Edinburgh thesis of 1837, the date and complete title of that work being given. Probably Dr Couty has not extended his researches beyond the literature of France. His scanty references to works in foreign languages seem to have been obtained at second-hand; and his sole acquaintance with the writings of Sir John Rose Cormack on Air in the Veins appears to have been derived from a cursory perusal of that author's Paris thesis of 1870, entitled—"*De l'Entrée de l'Air par les Orifices béants des Veines Utérines considérée comme cause de Danger et de Mort subite peu de temps après la Délivrance.*"

We have not space to give extracts from, or an analysis of, Dr Couty's work; but we commend it to the notice of our readers, as containing some useful information on the subject of which it treats. The author adopts the mechanical cardiac theory; agreeing therefore with Nysten, Cormack, and most later authors. He also concurs with Cormack in believing in the efficacy of bleeding, and cites in support of that opinion the case treated at Barnes in 1850; and which is reprinted from the *London Journal of Medicine* of that year, in the second volume of Cormack's "Clinical Studies," a most important case, to which we called particular attention when reviewing that work in December last.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH

### SESSION LVI.—MEETING II.

*Wednesday, 6th December* 1876.—Dr H. D. LITTLEJOHN, *Vice-President, in the Chair.*

I. THE following patient was shown:—

*Mr Bell* showed a young lad on whom he had operated for a LARGE STRANGULATED INGUINAL HERNIA. The hernia had existed for a year, but had always, up to above date, been reducible. The ordinary operation for relief of the stricture was performed. When the bowel was returned, it was then noticed that the sac was large, and with a large opening as well. He was unwilling to leave the patient in this state; and he therefore, with antiseptic precautions, ligatured the neck of the sac with catgut. The wound healed perfectly, and now the members could feel that the impulse was less on the injured than on the sound side. The sac had atrophied. The truss, of course, was still worn, and would be so for a year. The ligature he believed had set up as much inflammation as to mat all the structures at the opening together. A little of the cellular tissue came away as slough, and there was also some scrotal inflammation. He hoped the sac would be obliterated. In conclusion, he believed that this operation, from the adoption of antiseptic precautions, would be still more common.

In reply to a question from Dr Littlejohn, Mr Bell explained that the ligature did not come away, but remained in the tissues, having undergone organization.

II. *Mr Annandale* showed (1) PHOTOGRAPHS of the patient suffering from the disease "OSTEITIS DEFORMANS," whose case had been described by Sir James Paget at a recent meeting of the Medico-Chirurgical Society of London. He had been in Windermere seeing this patient with Dr Hamilton, at the beginning of this year, and, believing the Society would be interested in a short account of the case, he had written and obtained Sir James Paget's permission to show these illustrations. The photographs, for which he was indebted to Dr Hamilton, showed the great hypertrophy of the bones of the skull. This in no way interfered with the patient's intelligence, although it caused great deformity. They would see from the photograph the cap formerly worn by the patient, viz. that of a yeomanry officer, and the cap now worn by him, and would then get some idea of the extent of the hypertrophy. The curving and hypertrophy of the bones of the limbs would be clearly seen in the second photograph. This disease had existed for twenty years. What he had been consulted about was malignant disease of the bones of the forearm, accidental and unconnected with the above malady.

*The Chairman* asked whether this *osteitis deformans* was really a new disease, or only an old disease with a new name.

*Mr Annandale* explained that it was a form of disease first described and named by Paget. A good account of Sir James's paper would be found in the *Lancet* of 17th November. (2.) A HALFPENNY which had been lodged in the ŒSOPHAGUS of a child, five years old, for three or four weeks. It had passed down while the child was playing with it. There was pain and difficulty in swallowing for the first three days, but after that he went about as usual The mother, however, was anxious, and came to hospital to consult him. After chloroform was administered, he passed a metal bougie down the œsophagus as far as a few inches below the top of the sternum, but could find no trace of the coin. He then passed an ordinary œsophagus bougie down to the cardiac end of the stomach. As he withdrew it, he felt it catch something. By bringing up the bougie with a wriggling motion, and by the help of his finger at the back of the pharynx, he got the coin out. The result was thus satisfactory, although accidental. In connexion with this case he showed the newest forceps for such attempts, which he had just got from Paris. They could reach down to the stomach, whereas ordinary ones only went as far as the sternum. They were a modification of what he had seen his grandmother use for picking up objects from the floor. (3.) TWO ENTIRE TONGUES removed by the new thermo-cautery of Dr Paquelin. It had the advantage of simplicity over the ordinary galvano-caustic apparatus. The powerful galvanic battery in connexion with the latter was constantly going out of order, but this new instrument was both simple and efficacious. He had brought the tongues to show that they could be removed by it easily, and without hæmorrhage. He had also used it in ulcerated mammæ and in large epitheliomatous tumours. Where in such cases there had been great pain prior to the operation, he had found that after the operation complete freedom from pain resulted. This had also been the experience of Dr Matthews Duncan in connexion with the galvanic cautery. (4. Preparations illustrating EXCISION OF THE HEAD OF FEMUR. In the first specimen they would see that the entire cartilage was gone. Although there was no external opening, he had yet, on the principle that incision should be made where there was suppuration, cut down into the joint. He then found that there was a hole through the acetabulum into the pelvis. Unless, therefore, he had excised the head of the bone, pus would have burrowed in the pelvis. By the operation, however, a free external drain was given. The second specimen illustrated that form of the disease which began in the epiphysis, between the head and neck of the femur, and which usually resulted in necrosis. There was no external abscess, but yet he had found the head of the bone separated and part of the neck loose. No treatment short of excision would have sufficed here. The necrosed part would have acted as a foreign body, and caused continued suppuration, until the dead bone was removed.

III. *Dr W. A. Finlay* showed a CAST taken from a patient who was admitted on 5th Dec. into the Leith Hospital. It was a case of dislocation of the patella edgeways. The inner edge of the patella was felt protruding under the skin, and the outer edge was fixed on the femur. The slightest movement of the joint caused extreme pain. Chloroform was therefore given, and whilst he flexed the thigh on the abdomen, the leg being semi-flexed, the bone at once slipped into its place without being manipulated. The accident was caused by direct violence, the knee having been jammed between two barrels. He was indebted to Mr Daruty, one of the horse-surgeons, for having promptly taken this cast of the dislocation on the patient's admission.

Several of the members commented on the rarity of such a dislocation, and the value of the cast so promptly taken.

IV. *Dr Hunter*, Linlithgow, then read NOTES OF AN EPIDEMIC OF TYPHOID FEVER, WITH SPECIAL REFERENCE TO ITS TREATMENT BY ANTIPYRETICS, which will appear in a future number.

*The Chairman* had listened with much pleasure to Dr Hunter's admirable paper, and felt fortunate in presiding over the meeting assembled to hear it. He could certainly bear record to the accuracy and diligence with which Dr Hunter had collected his facts. In his visits to the village, nothing struck him more than that in every house there were charts for accurate record; wherever he went he found Dr Hunter and his assistant anxious to treat the outbreak in a scientific manner. No expense nor toil was spared, and he felt sure that no epidemic had ever been treated more successfully. How it was accomplished he did not know. Although

Dr Hunter's practice was large and the calls on him many, yet late and early he found him at his work. He could vouch for the accuracy of the whole paper as to its description of the village itself, and the history of the epidemic. He was quite at a loss to account for its beginning. He was astonished to find, on inspection, that, in this dirty hamlet, the outbreak began in the only house of the nature of a villa in the place; the sanitary arrangements of this house were almost without a flaw, unless that the ventilation was somewhat defective. When he visited Braefarm he found churning going on within a foot and a half of a bed with two adults in it affected with the fever. When he entered the room, the peculiar febrile odour was so marked that he felt compelled, in due regard to his own preservation, to retreat at once. Dr Hunter had given it as his belief that the first case contracted the disease in Edinburgh. He doubted whether this could be the case, because although typhoid fever was endemic in Edinburgh, yet it did not act as a focus of contagion. If Dr Hunter's view were true, then typhoid fever might spread over the whole of Scotland, as Linlithgow bridge stood on one of its principal highways. He was glad to find that there were present so many gentlemen practically acquainted with the subject, and he felt sure they would all specially welcome any remarks Professor Gairdner wished to make.

*Mr D. J. Hamilton* had noticed that Dr Hunter, in the beginning of his paper, had raised the question of the origin of typhoid fever. It was generally supposed and held that it was propagated by water or by actual contact of contagious material from dejecta. As to this he had an opportunity last summer of examining an interesting case where the mode of origin was probably different. A family with three children came to reside in a house near Edinburgh, which had been unoccupied for some time. The three children were put into a room in a sunk flat, and a fireplace was fitted up in it. After being in the house for about three weeks, they were all seized with typhoid symptoms. Two died, and on one of these he made a post-mortem examination. It came out afterwards that there was an old obstructed drain in the neighbourhood, from which sewage soaked through the walls, so that, when a fireplace was made, emanations filled the room. The water used was the ordinary water supplied to the town. On making the post-mortem of the one child, he found that although the illness had only lasted for three days, there was yet a lesion of a diphtheritic nature of all the solitary glands and Peyer's patches; all the small intestine was covered with a yellow slough, consisting of shed epithelium. He examined it microscopically and found numbers of large spores greenish in colour, and certainly, from their mode of division, vegetable in nature. He could not say whether or not the green colour was due to bile-staining. He placed some of these in Pasteur's solution, keeping it at a suitable temperature, in order, if possible, to cultivate the fungus; but the only result was a copious growth of Aspergillus niger. He wished to ask Dr Hunter if any microscopic examination of the water had been made. At Breslau, the water of a well, famous for the production of typhoid, was found by Cohn to contain a peculiar alga, the Crenothrix polyspora, whose spores were said by Klein to resemble those found by him in typhoid. In typhoid fever he had always found micrococcus abundant on the surface of the intestine, but whether it was of a specific nature or not he could not say. The appearances were almost identical with those found in diphtheria.

*Professor Gairdner* did not feel in a position to say much as to the excellent paper, except in regard to the remedies employed. As the Chairman, however, had been good enough to ask his opinion, he felt bound to say that he concurred in the praise awarded to the paper just read. It was an admirable sketch of an epidemic; and whatever might be the ultimate view as to the use of salicylic acid and salicylate of soda, yet no one could say but that Dr Hunter's treatment was most scientific. He had nothing to contribute, as regards typhoid fever, except the record of one case. In this instance the temperature had risen in two distinct relapses after it had gone down. Thus on the forty-first day, when it was 104·4°, he gave the salicylate of soda. The temperature was taken every three hours, and showed a rapid depression by about 5° in a few hours. Unfortunately so much disturbance in stomach and bowels followed, that when the temperature rose again, no drug was given, and it fell spontaneously. He had given salicylate of soda and salicine in a number of febrile cases, such as febrile phthisis, remittent fever, and so on. The opinion he had formed, and which he wished to put as an hypothesis, was that he was not so sure that in reducing the temperature they did so by removing the disease. In acute rheumatism he thought a strong case had been made out in favour of salicylic acid, but, after reading Dr Maclagan's paper in the *Lancet*, he had gone back to salicine. On looking over his experience he felt sure that, by such antipyretics, he would have done good and saved life, when death was caused by the high temperature. In view of the excruciating torture a patient suffered in acute rheumatism, he thought such drugs as salicine and salicylic acid much better than the cold bath. He believed, in conclusion, that the whole subject was not in a settled state, but needed working out.

*Dr C. Muirhead* felt interested in the therapeutics of Dr Hunter's paper, especially as to the antipyretics, and he wished to give his experience of this method of treatment as pursued in the fever wards of the Royal Infirmary of this city. When first he took charge of these wards, seven years ago, he attempted to reduce the excessive temperature by means of the cold bath. This method he very speedily found was impracticable, because of the large number of baths required for each patient per diem, and the limited number of attendants. Consequently, he turned his thoughts to those remedies, which, when given internally, are known to exert an antipyretic influence. And the one which he selected was quinine. And the method pursued was to give the drug, in large dose, some hours prior to the time when the temperature attained its maximum (between seven and nine o'clock P.M.). Within four hours after the exhibition of the remedy, the temperature showed a marked fall, continuing to decline till the lowest point was reached, about seven A.M. After this it again gradually rose, seldom, however, rising to so high a point as before the dose was given. If it did reach this or exceeded it, then twenty-four hours after the first dose, another, still larger, was given. Usually, however, it was found sufficient to repeat the original dose forty-eight hours after the first. As to the dose, he found that one large dose, thirty grains, acted much more efficiently than when the drug was given in frequently repeated small doses. Cinchonism was rarely induced by this method of administration. The patient occasionally feels somewhat excited after the dose, but speedily he falls asleep, perspiration breaks out over the whole body, indeed it rolls off him, often saturating the bed linen, and he wakes greatly cooler, with perhaps a little deafness, but no headache. This treatment is now always pursued in cases of enteric fever, whenever the temperature rises to 103° F., and the result is almost invariably a fall of 5° or 6°. In a few cases, quinine is not tolerated, but even where it is rejected by the stomach, its antipyretic influence is well marked. The fever is thus changed in character. From being a continued fever it is transformed into an intermittent fever, thus rendering the risk of death to the patient, from the effects of the fever, infinitely smaller. Since this method of treatment has been adopted, the mortality has been reduced in an extraordinarily marked degree. As to the other antipyretics which he had tried, —viz., digitalis, salicine, salicylate of quinine, salicylate of soda, salicylic acid—none of these exhibited the same power in reducing the temperature of enteric fever as did quinine. He could not, in conclusion, sit down without expressing his admiration at the time, labour, and skill displayed by Dr Hunter, notwithstanding his arduous duties elsewhere.

*Dr G. W. Balfour* said that, while the expectant and antipyretic treatments of enteric fever had been discussed, no reference had been made to the antiseptic treatment. He had employed this treatment, using sulphurous acid internally, for many years, having commenced it in the fever wards of the Royal Infirmary previous to Dr Muirhead's appointment, and continued it ever since. His experience of this treatment was, that if commenced early enough, a rise of temperature over 103° was almost unknown, and the few deaths which had occurred had been due to hæmorrhage. Quite recently, in one case, the temperature had risen to 104°, and kept there for two days in spite of the antiseptic treatment; the case was otherwise a sharp one, and, fearing for the safety of the patient, he ordered a large dose of quinine. Next day, he was pleased to find that, the patient having improved towards evening, the antipyretic had not been given, yet the temperature had fallen, and did not again rise above a tenth or two above 103°. His experience was, that while antipyretics undoubtedly were capable of inducing a very considerable fall of temperature, this was very shortly followed by a rise to the original dangerous degree; but that the antiseptic treatment, if commenced early enough, kept the temperature low, and favourably modified all the symptoms, at the same time disinfecting the stools at least, if not also all the other secretions, while, by rendering the stools acid, it diminished their frequency by lessening the irritating effect on the intestinal mucous membrane due to alkalinity. As to the mode of origin of the first case, he was inclined to agree with the President that in all probability it had originated *de novo* from pythogenic poisoning, the result of unventilated water-closet arrangements, which all experience had shown to be pre eminently dangerous in the production of such diseases, and was not picked up in Edinburgh. He had noted the origin in a case of diphtheria, which, as Mr Hamilton had shown, was analogous to typhoid. On paying a visit to a farmhouse in a certain village—one of the healthiest in Scotland, where hitherto there had been neither typhoid fever nor diphtheria—he found that a case of diphtheria had occurred. No reason could be given for it; there was no middenstead, nothing but the free breath of heaven. It turned out, however, that in the house where it happened—an Irishman's, and the dirtiest in the village—the farmer had introduced a sink communicating, by means of an untrapped drainage-pipe, with a manure tank. He wished, in conclusion, to express the pleasure he had in listening to the paper.

*Dr Foulis*, in following the remarks of the speakers on pyrexia, had been struck by the absence of any allusion to its treatment after surgical operations. Dr Hunter's remarks on treatment by quinine, salicylic acid, salicin, etc., had reference to typhoid fever. But there was a whole host of cases, viz., pneumonia, pyæmia, etc., in which the question might be asked, How would quinine, etc., act? Would they act on the cause of the high temperature, or on the nerve-centres whose alteration caused the increase? He believed that their whole good depended on the answer given. He had lately two cases of high temperature, viz., one in a case of puerperal fever where the temperature was 110° F. before death, and indeed may have gone higher. In the second case, the high temperature followed the tapping of an ovarian cyst. Salicylate of soda in 15-grain doses caused vomiting, and therefore Dr Keith removed the cause of the high temperature, viz., a suppurating ovarian cyst, and the temperature fell. In reading Liebermeister's cases, he was struck by his saying that the object was not to treat the disease, but to reduce the high temperature, and thus allow the heart and brain of the patient to carry him through the disease. It was therefore essential to know whether the drugs given reduced the temperature by acting on the specific poison or on the nerve-centres. He had raised this question, as it was not right to give remedies without knowing their mode of action.

*Dr Hunter* thanked the members for their kind criticisms. He would only reply to Mr Hamilton's question,—no microscopical examination of the water was made. The chemical examination was conducted by Drs Stevenson Macadam and Falconer King. As to the reduction of the temperature for the purpose of allowing the heart, etc., to carry the patient through, they should remember that it had been found by experiment on dogs that when the temperature of the blood was greatly increased, such changes took place in the liver, brain, etc., that life could not be carried on.

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXXV.—MEETING XI.

*Wednesday, 14th June* 1876.—Professor SIMPSON, *President, in the Chair.*

Minutes of the previous meeting having been read and approved, the adjourned discussion on *Dr Macdougal's* paper on EXTRA-UTERINE GESTATION was taken up.

I. *Professor Simpson* exhibited two preparations illustrative of the subject of EXTRA-UTERINE PREGNANCY. The first he owed to the kindness of Dr Will, of Aberdeen, who had published the history of the case in the Edinburgh Monthly Journal for 1855. The patient, aged 39, had conceived shortly after her marriage in 1844. Two months subsequently she had symptoms of pelvic inflammation and threatened abortion. From this she rallied and went on to her full time, when labour pains came on, but passed off again. Thereafter, the tumour, which had steadily increased in size till then, shrunk, and the patient's health was re-established. Seven years later, in January 1851, Dr Will attended her in her delivery, after a natural gestation and labour, of a female child; and felt then the firm and partially movable tumour lying to the left side. In February 1854, the lady gave birth to a second full-time child, a male, after an easy labour. On the third day after, a rigor set in, followed by peritoneal inflammation; and, after a prolonged illness, attended with hectic and diarrhœa, the patient died in May. At the dissection of the body, the fœtus now exhibited was discovered lying in a cyst, the walls of which were adhering to all the surrounding parts, and in the cavity of which some fæcal matter was found that had got access through three different orifices in the bowels. It was an illustration of an extra-uterine fœtus carried for many years in a patient who gave birth to other children, and at last leading to her death under the efforts of nature to effect its elimination.

The second preparation was one of a macerated fœtus of about the third month of pregnancy, measuring three inches in length. He had only once seen the patient from whom it was removed, along with Dr David Gordon, who was present and could supplement the history of the case. The woman had an obscure history of having conceived and aborted, and at the time when he (Prof. Simpson) saw her she seemed to be dying of extravasation of blood into the peritoneal cavity. She was pale and pulseless, with a fluctuating swelling reaching half way up the abdomen, and the propriety of making an incision to give an outlet for the blood through the vaginal roof was discussed. It was, however, set aside, chiefly because of the seemingly moribund condition of the patient, who was treated by applications of ice to the abdomen, and opiate and stimulant internally. She rallied, and for a time seemed to recover satisfactorily; but died some months later from an attack of acute peritonitis. At the post-mortem examination made by Dr Wyllie, Pathologist to the Royal Infirmary, the peritoneal surface was seen to be pigmented in the lower part of the abdomen cavity as high as the level of the umbilicus. There were evidences of active inflammation among the coils of the intestines above the pelvic brim, and in the pelvic cavity amidst a fæcal and putrilaginous mass was found the preparation now exhibited. It lay in the space of Douglas, at the bottom of which it was discovered that perforation into the rectum had taken place. Through this orifice liquid fæces had escaped into the serous cavity, and seemed to have set up the fatal peritonitis.

He had seen another case of extra-uterine gestation, of which he could not show the preparation, as it had been kept for the College of Surgeons Museum by the then Pathologist of the Infirmary, where the patient had died. The woman had first come into his ward suffering from some pelvic distress, which was associated with a swelling rising above the pubes towards the left side. He satisfied himself that the fœtal heart was to be faintly heard through the stethoscope, and thought it a case of pregnancy complicated with a uterine or ovarian tumour. Dr Slavjansky and some others who saw the case doubted the existence of pregnancy; and as, after a few days' rest, the patient, a married woman, with other children, felt rather easier, she was allowed to go home under promise to return in two months. She did return, but not for ten months. The swelling had grown steadily, she felt fœtal movements, and at the full term had symptoms as of labour beginning. When these passed she missed the movements, and three months after began to suffer from pain in the abdomen, which induced her to come again to the ward. The tumour was now felt occupying the abdominal cavity; but the patient was suffering from so much peritonitic tenderness that a satisfactory exploration of its relations could not be made out. He thought he could feel the outline of a fœtus, and a small aspiratory tapping brought away an ounce of fluid resembling liquor amnii, only darker than usual and with much cholesterine. The peritonitic symptoms increased in severity till the patient died. At the autopsy there was found to be very extensive peritonitis, flakes of lymph lying among all the abdominal viscera up to the diaphragm. The walls of the tumour were matted to the abdominal parietes and to all the viscera which lay around it. On being opened into it was seen to be one of those rare cases where the gestation of the ovum had occurred in a Fallopian tube (the left), and where the tube had gone on developing to the close of the ordinary gestation. The question of operative interference had been discussed before the University Clinical Class, to which the case had been demonstrated as one of probable extra-uterine gestation; but experience showed that, after gestation was ended, operative measures were likely to be satisfactory only in cases like that of Dr Macdougal's which had led to these remarks,—cases, viz., where nature had already commenced, and to some extent carried on, the process of elimination.

II. *Dr Mossop* of Bradford read the account of a successful CASE OF OVARIOTOMY.

*Dr Underhill* thought Dr Mossop's case interesting, more especially as it had been successful. The chief peculiarity was the sessile nature of the pedicle, and the means adopted to secure it, which had proved quite satisfactory in the result of the whole case. He congratulated Dr Mossop on its favourable issue.

*Professor Simpson* thought the Society were much indebted to Mr Mossop for the interesting narration of his successful case. He had himself seen most of the well-known ovariotomists of Britain at work, and had seen the pedicle treated in various ways, and had come to the conclusion that, apart from the favourable condition of the individual case, the success of the operation depended less upon the choice of any particular method of dealing with the pedicle, than upon the care with which each step of the operation was carried out, and the subsequent treatment of the patient conducted. He had operated thrice. In the first case he had operated only at the urgent desire of the patient, who was beginning to sink after a second tapping. There were, as he had anticipated, very extensive adhesions, which made the removal of the tumour very difficult and tedious. The pedicle was clamped. The patient died of peritonitis in three days. In the second case all the conditions were entirely favourable. The tumour was easily drawn out through a short incision. The pedicle was seared, and the patient recovered without a drawback. In the third case the adhesions to the abdominal parietes were pretty extensive, though no vessels of any size were divided during their separation. He had hoped to secure the pedicle with the cautery; but after careful searing, when the clamp in which the pedicle had been caught was relaxed, oozing of blood occurred, and he had been obliged again to clamp it. That patient had also recovered without ever showing any local distress or any febrile reaction. When the hæmorrhage could be arrested by means of the cautery it certainly gave very satisfactory results, and the use of it was free from some of the distress and anxiety attendant on the employment of the clamp. He hoped that Mr Mossop would be encouraged by the result of his first operation to repeat it in other cases, and that he would favour the Society with further reports of his success.

*Dr Dickson* mentioned that he had twice delivered a patient who had been operated on for ovarian tumour.

*Dr Mossop* asked permission to be allowed to mention a case of death from hæmorrhage in a child after the separation of the cord. He asked the opinion of the Society upon the case, and others of a like nature. A short discussion then took place on the subject, in which the President, Drs Ritchie, Burn, and Gordon took part.

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## SESSION XXXVI.—MEETING I.

*Wednesday, 8th November* 1876.—Professor SIMPSON, *President, in the Chair.*

Previous to the opening of the meeting the Fellows and a number of visitors partook of tea and coffee in the adjoining room. Thereafter, the Society having been constituted, the President briefly introduced Professor Turner to the Meeting, stating that he was sure the Society would heartily endorse the step taken by the Council in asking Professor Turner to inaugurate the present session by addressing them on a subject regarding which he had made very important researches,—the Anatomy of the Placenta.

*Professor Turner* then gave an able and instructive SKETCH OF THE ANATOMY OF THE PLACENTA, embracing the researches which he had himself made within the last few years both on the human placenta and that of other mammals. Towards the close of the lecture he referred to many interesting points in the physiology of the organs, more especially the placental circulation, and pointed out how recent advances in our anatomical knowledge were helping to clear up many disputed points in the physiology of the placenta.

*Dr Keiller*, P.R.C.P., at the conclusion of the address, proposed a cordial vote of thanks to Professor Turner, and, in doing so, suggested that, in recognition of Professor Turner's present and past favours conferred on the Society, he should be elected an Honorary Fellow.

*Dr Matthews Duncan* cordially seconded the proposal of Dr Keiller. He characterized the researches of Professor Turner as highly important, as having greatly added to our knowledge of the placenta. The value of an accurate anatomical knowledge of the organ could not be overestimated, as it was only on such a basis that in practice we could establish any rational principle of treatment with regard to the placenta. He pointed out that, although so much had now been made out in regard to the ordinary anatomy of the placenta, much yet remained to be done in regard to the minute anatomy of the organ.

*Professor Turner* having expressed the gratification which it had given him to address the Society that evening, the meeting adjourned.

## SESSION XXXVI.—MEETING II.

*Wednesday, 22d November* 1876.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Matthews Duncan* read a paper on A CASE OF SO-CALLED TORSION OF THE CORD, and showed a preparation of the cord in illustration.

L. S., age 20, unmarried, admitted to the Royal Maternity Hospital, 10th October 1876, stated that she last menstruated in the end of April; had not felt life for a week previous to admission; and had been ill since the evening of the 8th.

Her face and body were studded with syphilitic maculæ, which, she said, appeared during the third month of her pregnancy. On the vulva were three sores in process of healing. The throat was healthy.

At 11.45 on 10th October, the os admitted the finger-tip, but was somewhat rigid. The fœtal heart could not be detected.

At 1.30 A.M. on 11th, the child was born; it presented the breech, and appeared to have been dead about a week. The cord at the umbilicus was twisted not more than one and a half times upon itself, and atrophied. Around the umbilicus was an irregular reddish-brown discoloration, extending chiefly upwards and to the right. The placenta was apparently healthy, as were the cord and its vessels. The umbilical vein was in parts thrombosed. The liver, spleen, and thymus gland showed no evidence of disease. The bones, and more particularly the femora, showed a morbid condition of the ends of the shafts, and of the adjacent cartilages.

The atrophied portion of cord in this case began at the umbilicus, and extended for a quarter of an inch of the length of the cord. The vessels leading to and from the atrophied portion were dissected by Mr Graham, and found healthy. Around the insertion of the cord the skin of the abdomen was diseased, the affection having probably been inflammatory. The twisting of the cord at the affected part was certainly not more than one and a half turn.

*Dr Matthews Duncan* could not regard torsion as the cause of the atrophy of the cord and obliteration of the vessels, but considered the atrophy the paramount lesion. The rest of the cord showed no sign of twisting, and even the affected part was very little twisted, although at first sight it had the appearance of being considerably so.

*Dr Keiller* asked if Dr Duncan had brought forward the case as an evidence that torsion could not cause the death of the child. He thought it was important to consider twists anatomically. Twisting of the cord in many cases was not so great as it appeared to be. He had seen cases of death of the child from twisting independently of disease.

*Professor Simpson* asked Dr Duncan the length of the cord, because he had found that in cases of true torsion, the cord was usually of more than average length, and he thought the case narrated by Dr Duncan interesting as bearing on the observation made by Virchow and Mr Lawson Tait as to the existence of small capillaries in the umbilical extremity of the cord. These were regarded as playing an important part in the nutrition of the cord, and it was important to remark that, in this case of atrophy, there had been a diseased condition of the skin around the umbilicus.

II. *Dr Milne* read a paper on POST-PARTUM HÆMORRHAGE, with cases; and reference to the revival of old errors as to causation. In reply to a question from *Dr Bruce*, Dr Milne stated that the tampon he used was a sponge inserted into the os uteri.

*Dr Matthews Duncan* thought it certain that Gooch was wrong in stating that a contracted uterus might bleed. The case which Gooch gave was not reliable. Gooch was correct in saying that a relaxed uterus did not always bleed. The difficult point in the pathology of bleeding was one on which he and others had made observations. He referred more particularly to the retentive power of the abdomen, and the relation of the blood-pressure in connexion with it. The question was, what is the cause of the varying amount of blood-pressure in different individuals? In relation to this, he referred to the fact that, in cases of amputation of a limb, the veins did not usually bleed, and yet from a small opening in a varicose vein a woman might bleed to death. In his opinion, the difference depended on some morbid relation of blood-pressure in the vessels. In regard to sponge-plugging in post-partum hæmorrhage, he did not approve of it.

*Dr Macrae* had heard Dr Milne's paper with pleasure, and in regard to most points he agreed with him. He was at one with Dr Matthews Duncan in considering the importance of taking into account the effects of blood-pressure as a cause of hæmorrhage. He referred to cases he had met with in which apparently a loaded colon had been a fruitful predisposing cause of bleeding; also a congested liver, as evidenced by signs of enlargement of the organ, which had been relieved by calomel and podophyllin. He had lately found benefit in cases of post-partum bleeding from the use of a new preparation of ergot and ammonia.

*Dr Keiller* considered the paper an important one. Inexperienced practitioners often thought a case one of hæmorrhage, when, in reality, it was not by any means so serious as was supposed. He was of opinion that in many cases patience and non-interference were necessary. In practice it was sometimes difficult to ascertain if the uterus was really contracted. He alluded to the value of compressing the uterus from above, and he rather avoided putting his hand into the uterus, but kept it in the vagina outside, spreading out his fingers around the cervix, and compressing the walls of the uterus. He did not approve of plugging or injections. We were not to judge by the actual amount of blood lost, but rather by its effect on the patient.

*Dr Bruce* referred to several of the predisposing causes of hæmorrhage, more especially that of allowing the labour to go on too long without interference. This, in his opinion, was a most common cause. Another was the removal of the placenta too quickly. In all cases he advised delay until the uterus had begun to contract, unless this action be too long deferred, or symptoms requiring more immediate interference should supervene.

*Dr James Carmichael* asked if, in the experience of any of the Fellows, the administration of chloroform during labour had tended to produce more than usual relaxation of the uterus after delivery. In some cases he had fancied this had been the case. He commended the practice of Dr Beatty in recommending that ergot should always be given in the second stage when chloroform was administered. He referred to a case of post-partum hæmorrhage he had lately seen, in which ergot and manual compression had failed to keep up uterine contraction. In this case he put the baby to the breast, and the effect of pulling the nipple was most satisfactory; all bleeding very soon ceased, and the patient, from being in a semi-fainting condition, rapidly recovered. He thought this well-known hint of nature should not be lost sight of, and that, when other means failed, squeezing or pulling and irritating the nipple, either artificially or by putting the child to the breast, should be had recourse to.

*Dr Balfour* asked if any of the Fellows had had fatal cases from hæmorrhage in their own practices. Dr Matthews Duncan had not had a fatal case in his own practice, but had frequently seen them in the practice of others. Dr Balfour thought the opinion Dr Matthews Duncan had expressed, with reference to the analogy between the bleeding in cases of amputation and that in the uterus, was not correct.

*Dr Rattray* mentioned a case of flooding in a case of placenta succenturiata. He remarked that Dr Milne had not made reference to the use of the pad. He thought it of great service. Ergot he had found of signal advantage lately in a case of hæmorrhage. He did not approve of plugging; he had often recourse to the application of ice with benefit.

*Dr James Young* drew the attention of the Society to a paper in the

*Practitioner* by Dr Haddon of Eccles, in which it was stated that in no case of hæmorrhage did the contraction or relaxation of the uterus affect the bleeding. Dr Y. considered, in regard to treatment, that the means already adopted, viz., pressure externally and plugging internally, were quite satisfactory as well as sufficient, and that no new method or treatment was required.

*Dr Gordon* approved of use of the sponge in cases of bleeding from lacerations about the os and cervix; and, in connexion with this, *Dr Milne* observed that he did not introduce the sponge into the interior of the uterus.

*Prof. Simpson* alluded to the frequency of death from hæmorrhage. He considered the quantity of blood lost a matter of much moment. He had seen cases in which the patient was literally drained to death. A woman, in his opinion, was always the worse of large losses of blood during labour, and her health was often seriously affected by it in many ways afterwards. He had lately seen a case with Dr Croom, in which the patient was delivered of twins, and serious hæmorrhage followed; ergot, ergotin injections, and all the usual means had been tried without a satisfactory result. After holding the uterus for half an hour the bleeding stopped, but the patient never rallied, but died in forty-eight hours. As to causation, besides the question of blood-pressure, some importance attaches to the site of the insertion of the placenta. When the placenta was situated flat on the anterior or posterior wall, the mere weight of the uterus acted in assisting to restrain hæmorrhage; in cases of placenta succenturiata, and in lateral or low insertion, there was more likelihood of bleeding. Cold-water injections he had found frequently of service.

*Dr Milne* replied. He was much pleased by the manner in which his paper had been received. The discussion had elicited very little difference of opinion on the main points; all, for example, were agreed in the essential importance and value of uterine contraction, on which he (Dr M.) strongly insisted. He was greatly gratified by Dr Duncan's renewed protest against the absurdity of viewing lightly, or even neglecting entirely, the utility of said contraction. His friend Dr Keiller, whom he highly respected for his great practical experience and skill, had referred with condemnation to the use of the tampon. Now he (Dr M.) would join Dr Keiller in condemning it as strongly in the majority of instances, but he had not made up his mind to discard it in those cases, as he said, happily rare, where the only alternative was the styptic injection. He had the authority of the President for saying that his uncle, the late Sir James Simpson, had in at least one case employed the tampon. He would repeat, that it must be carefully and judiciously done, and the uterus should be watched well and manipulated externally at the same time. If uterine relaxation and expansion took place, then it would be our bounden duty to extract the plug at once, and to resort, if need be, to the desperate measure of Barnes.

# Part Fourth.

## PERISCOPE.

On the Application of the Nitrate of Silver to Ulcers. By James Cuthill, M.D.—When the solid nitrate of silver is freely applied to an ulcer, a tough film is immediately formed, and the ulcerated surface is for the time being apparently sealed up. The benefit to be derived from such a proceeding, however, as most surgeons who have seen a little practice well know, is only temporary, the pellicle becoming detached by the ulcerative process, leaving a sore frequently larger than the original one. A better plan, which I have practised in some recent cases with excellent results, is merely to score the ulcer with a finely pointed pencil of the nitrate, or only to dot it lightly, at intervals, on the surface. The discharges getting free vent from the non-causticated points, no sloughing occurs, and a healthy pellicle spreads from the touched portions, just as ice forms on a pond of water.

Spindle-celled Sarcoma.—Dr Satterthwaite, in a memoir, in the *New York Medical Journal* for July 1876, *On the Development of Connective Substances*, explains the histology of spindle-celled sarcomata in the following way: After isolating the seeming spindle-cells in Müller's fluid or ten per cent. salt solution, he introduced a current between the glasses, and caused them to roll over. "It may then," he remarks, "be seen that they are long, flattened, and of irregular size, appearing on profile view to be spindle-shaped, and yet we may often press off the nuclei by pressing the cover upon the slide, showing conclusively that such spindle-cells are really the intercellular substance at an early stage of fibrillation." This is an application to pathological histology of the belief, now held by a considerable number of authorities, that a spindle or fusiform cell is always a flat epithelial-like cell seen edgeways. Dr Satterthwaite, however, does not make the principle of universal application, as he further remarks, that "we sometimes see them where they appear to be composed of real spindle-shaped bodies, closely packed together, and where each body contains within it a smaller flattened body."

Martin. *On the Physiological Position of the Uterus.*—(*Zeitschr. f. Geburtsh. u. Frauenkr.* I. p. 375.)—It has been lately stated by several authors that there is a normal anteflexion of the uterus. Martin endeavours to show that this is not the case, except in periods of physiological transition. Except at these periods, the fundus uteri is not felt before nor behind the cervix unless pressure is exercised. There is a physiological anteflexion in childhood on account of the uterus itself being more easily bent. It disappears gradually in consequence of the greater development of the submucous layer, and only persists in girls of weak constitution. In 8528 women and girls Martin found displacements in 2325, whilst out of the total number in 3201 of the grown women (although most of them were suffering from uterine ailments), none could be found. Anteflexion must therefore be considered the exception, and not the rule.

In the third month of pregnancy the uterus is generally found anteverted, probably in consequence of the stronger development of the ligamenta rotunda which takes place at that time.

VOSS. *Is Syphilis Inoculable through Milk?*—(*Petersb. med. Wochenschr.*, 1876, No. 23.)—Voss inoculated three prostitutes with the milk of a syphilitic woman. This woman suffered from a papular syphilide; there were moist condylomata on the genitals and anus; the nipples were free. The milk was obtained by compressing the mamma, and a Pravaz (Wood's) syringe-full injected into each of the other three women. The first was already syphilitic, and the inoculation was naturally followed by no results. The second was suffering from urethritis, and remained unaffected. The third, 16 years old, had never had syphilis, was admitted into the hospital on the 16th September on account of urethritis, and had the milk injected on the 27th. A large inflammatory swelling followed (as also in the first patient), which suppurated, and was healed by the 24th October. On the 3d November (40 days after the inoculation) a papular eruption formed around the part which had been the seat of injection, and on the 8th November a maculo-papular syphilide with adenitis had developed on the rest of the body. The symptoms disappeared under mercurial inunctions. The author concludes that the milk is capable, like the blood, of producing syphilis.—*From the Centralblatt*, No. 44, 1876.

SUCCESSFUL INOCULATION OF CANCER IN DOGS.—Nowinsky of St Petersburg removed small pieces from a medullary cancer of the nose of a dog, and transplanted them into the skin of other dogs. Of these 27 were transplanted into inflamed skin, and all failed, but of 15 on healthy skin 2 succeeded, and tumours developed (in one case with glandular affection), which were shown by the microscope to be true medullary carcinoma. The possibility of inoculation of cancer would thus seem to be proven.—*From a Communication in Centralblatt*, No. 45, 1876.

BIDDER. *The Umbilical Cord felt through the Abdominal Wall.*—The cord was felt passing straight across the child's back at a point near the navel of the mother. It was slightly movable. In consequence of abnormal shortness, it did not slip off during labour, and the child was born dead.—*Petersb. med. Wochenschr.*, 1876, No. 8.

THE following piece of sound sense, well expressed, by Dr Clarkson of Morpeth, is well worth perusal:—

The monthly meeting of the Morpeth Rural Sanitary Authority was held at Morpeth, the Rev. B. P. Hodgson in the chair. The attendance of rural guardians was small. Dr Clarkson read his monthly report on the sanitary condition of the union. In July, 27 deaths had been registered, being an increase of three over the previous month, and of seven on the corresponding month of last year. The death-rate was 27 per 1000, a figure due to excessive infant mortality, represented by 15 deaths of children under five years, and 11 of infants under one year. He asked whence arose that excessive mortality among infants? For twelve months after birth, he thought the infant should hardly be said to have a separate existence from its mother. Its life was really wrapt up in, and inseparable from, that of its mother. Whether it lived or died during this helpless period depended altogether on the attention of its parents or guardians to cleanliness and regularity in the supply of nature's food. That would enable the infant to tide over that critical period, and enter its second year. Nature pointed them to a different kind of food, still sparingly to be given, and of mild and unirritating properties. As he was held responsible for the death-rate of this district, he asked this question of the parents or guardians of these eleven dead infants—Were they fed properly? He said nothing about the clothing or cleansing; he simply asked, were they brought up on the natural food? Alas! if Truth were to rise up that moment, she would tell they were stuffed, gorged, crammed with all sorts of indigestible food—in a word, with whatever the darling infant looked on; and in this way their lives had been sacrificed, not intentionally, but ignorantly. He was told that the high temperature had something to do with the deaths of these eleven infants. This was simply unmitigated bosh. How came it that in the western or agricultural division, extending over thousands of acres and thousands of population, the death of only one child was registered during the month? Surely the same sun did not strike infants dead at Easington and North Seaton, and spare them at Cambo! No; the question of infant mortality resolved itself into feeding, cleansing, and clothing, and was entirely, or all but dependent on the conduct of the parents. But there was another circumstance in relation to the deaths of these eleven infants, which he felt it his duty to bring before that authority, and that was the kind of skill brought to bear on the diseases of infants and others at most collieries. Itinerant quacks prowled about with their poisons, and young medical students, plucked by examining Boards, squatted down in mining districts, and, he dared say, killed far more than they cured. The owners of these collieries would not employ a quack engineer, a quack overman, or a quack weighman, and yet they would placidly permit valuable lives to be sacrificed daily by quack doctors. There was no use of facing him with the old hackneyed phrase that they could not interfere with the liberty of the subject, and the men were free to choose what advice they liked. He said no, not quack advice at any price, but certainly free to choose whatever legal qualified advice they thought fit. Here, he said, colliery owners were to blame, and very much to blame, for not stamping out such a class from their works, and they could do it with a word, and thereby materially reduce the death-rate. He asked the chairman, as the head of that authority, to use his influence with colliery owners to bring about some reform in this respect. In reply to a guardian, Dr Clarkson said one of the numerous quacks had told him that he (the quack) was educated at Felton. The meeting shortly after adjourned.

DETAILS OF A CASE, IN WHICH DEATH IS SUPPOSED TO HAVE RESULTED FROM ETHER USED AS AN ANÆSTHETIC; WITH A BRIEF ACCOUNT OF ALL THE FATAL RESULTS WHICH HAVE BEEN ASCRIBED TO THE USE OF ANÆSTHETICS AT THE PENNSYLVANIA HOSPITAL; WITH REMARKS. By THOMAS G. MORTON, M.D., Surgeon to the Pennsylvania Hospital.—David D. P., aged 19, single, a telegraphic operator, consulted me in May 1876, in regard to his right limb, which was quite useless from general atrophy, and an angular partial anchylosis of the knee, evidently a result of old scrofulous inflammation. I advised division of the flexor hamstring tendons, which were very much contracted, and immediate straightening of the limb, and directed an appropriate brace to be made, with a high heel and sole to the shoe for after use. On the 2d of June the patient was admitted into the hospital, when the following notes were made by Dr W. B. Hopkins, the resident in charge:—"Until the patient was two years old, when he began to walk, he had been considered a perfectly healthy child; soon after this a posterior spinal curvature developed; when four years old, inflammation of the knee set in, with subsequent contraction of the flexor thigh muscles, with general arrest of development in the limb, and subsequent partial anchylosis of the knee-joint, the angle of deformity being slightly obtuse; there had been, apparently, no suppuration of the joint, at least to the extent of an abscess."

"The spinal and limb troubles combined prevented the patient from walking until he was six years old, when, with crutches, he began to get about; there is no history of winter cough or previous chest trouble, and his general health, although not being vigorous, seems not to have been markedly impaired, and no evidence of any hereditary disease can be traced out. The patient is rather pale, but his appetite is good, and he expresses himself as in excellent health; there is marked posterior curvature, which has produced the usual deformity, with great sternal prominence. The right limb is much atrophied and shortened, and presents angular deformity, with a partial luxation backward of the tibia, the anchylosis not being complete. June 3, after a consultation with Drs Hewson and Hunt, the tendons were divided, and the limb was straightened, and the apparatus, made especially for the case, was adjusted by Mr Kolbé."

The etherization was conducted by Dr Wharton, one of the resident physicians; at the close of the operation I saw that the patient was pale, somewhat depressed, but as comfortable apparently as we generally observe after the use of an anæsthetic. The patient was left in charge of Dr Hopkins, whose subsequent notes are as follows:—"At 12.30 P.M., patient, having been removed 15 minutes ago to his room, was seized with symptoms of asphyxia; pulse moderately full, 160; respirations nearly ceased; general cyanotic condition more marked in the face and tips of the fingers; his tongue was at once depressed, cold water dashed on his chest, which produced only violent respiratory efforts. At 1 P.M. condition remains much about the same, and there being evidences of pulmonary engorgement, with frothy blood-stained mucus constantly collecting in the throat, while the heart's action was active and laboured, the radial artery was opened, and about 8 oz. of blood were drawn; dry cups were applied to the chest; for a time the respiration seemed slightly improved; the volume of the pulse increased somewhat, and fell from 160 to 152 per minute. At 1.45 rapidly sinking; hypodermics of whisky and carbonate of ammonia were used without avail." Dr H. sent me the following note:—"P. died at 2 P.M.; about ten minutes after the operation he was removed to his room, being perfectly conscious, and I having seen to it that he had perfectly recovered from the ether, he suddenly became asphyxiated; the ordinary remedies proved of no avail."

Dr Morris Longstreth, the hospital pathologist, made the post-mortem 21 hours after death. "The rigor mortis was well marked; there was considerable deformity of the chest; the lumbar portion of the spinal column was at its lower part bowed backwards, so that the last two vertebræ were nearly in a right line with the upper portion of the sacrum, thereby making the cavity of the pelvis very capacious. The abdominal viscera were normal, although somewhat out of position. Thorax—the pleural cavities contained a considerable amount of serum, included in the meshes of an abundant firm network of inflammatory adhesions (old). The serum contained no recent lymph, and it was impossible to determine its quantity, on account of the very great amount of similar serum pressed out from the lungs in the process of their removal; the lower part of the right pleural cavity was, to a considerable extent, obliterated by very firm adhesions existing between the diaphragm and the costal pleura. The amount of serum was not very great at any one portion of the pleural sac, but rather diffusely infiltrating the adhesions present. Both lungs were moderately well crepitant throughout, pitted deeply on pressure everywhere; on section, serum frothy and a little bloody exuded with great freedom and in large amount, as though from a saturated sponge; their cut surface was grayish, showing some blood points; there was no solidification or even deep congestion of their tissues. Divided portions as well as the whole floated well in water, and there was no evidence of plugging of their vessels, and the blood in them was entirely fluid. Pericardium contained a considerable amount of clear, very pale serum; the heart was only moderately in a state of muscular contraction, a slight amount of fat covered its surface along the grooves; the blood within the cavities was quite fluid; the muscular tissue, the valves, and the large vessels were all normal. There was no foreign substance of solid nature discovered in the larger bronchi or in the trachea. The other parts of the body were not examined."

The unfortunate result in this case, apparently caused by ether, naturally produced, not only upon myself, but upon those who had a knowledge of it, a profound impression, as it was seemingly a proper one for the use of the anæsthetic. Ether or "washed ether" as an anæsthetic agent has been uniformly looked upon as perfectly safe; personally, I have administered it on all occasions, at all periods of life, from infancy to extreme old age, with the feeble as well as in the strong. In giving ether I have never used any "inhaler," so called, but have invariably made use of a napkin or coarse towel, on which the ether is poured, when the cloth is then simply folded in a conical shape, and held over the patient's mouth and nose; so soon as the napkin becomes the least soiled or "ether-logged," a fresh one is substituted. Dr Wharton, the resident who gave the ether in this case, says:—"I am satisfied that not over two and a half or three ounces were administered in P.'s case; he seemed to be readily affected, and was not under its influence over twenty minutes. There was no vomiting, and the respirations were not laboured; on removal of the napkin he rapidly regained consciousness." Although this patient had a marked spinal curvature, with chest capacity below the average, anæsthesia on this account was not contraindicated, for I have frequently used ether in cases where the deformity of the chest was much more marked without the least difficulty. The autopsy demonstrated that the lungs were seriously restricted in their natural movements by the adhesions which bound them down in all directions, and consequently the secretions which formed were not expelled; at the close of the etherization there was no evidence of any mucous collection, at least when I last saw the patient at 12 M.; fifteen minutes later, when Dr Hopkins was called, the change was very marked, and it was during this short interval that the serious symptoms were manifested. Mucous secretion, which often rapidly forms, is one of the most annoying symptoms in the course of etherization; in many cases it is not observed at all, but when it occurs to any extent, it is much safer to suspend anæsthesia until the throat is thoroughly cleansed, or the patient is relieved by his own efforts. I have often observed that those patients who have been the most difficult to manage while being etherized, are the easiest to keep anæsthetized after the first excitement of the ether has passed over, and seldom suffer from any mucous secretions. Gross (Surgery, vol. i. 569, 5th ed.), when on the subject of death from ether, says:—"How the inhalation of ether proves fatal has not been decided; when death occurs promptly, as during, or very soon after, the administration, the probability is that it is caused by asphyxia, whereas, when it occurs at a later period, as after the lapse of hours or days, there is reason to believe that it is due to the effects of congestion of the brain and lungs, either alone, or in conjunction with gastric irritability, which often exists in a most distressing degree."

In P.'s case death was the result of the mucous secretion and serous effusion which invaded every available space, thus suffocating the patient; there was not the least evidence of any intracranial difficulty; the patient was quite rational to the close.

There is seldom any danger from ether when properly administered, and there is abundant evidence to show that fatal results are less likely to follow its use than any other anæsthetic agent. The fact of ether having uniformly been looked upon as a perfectly safe agent on every occasion may, I am inclined to think, have induced great carelessness in its use; the occasional deaths from, or at all associated with ether, should in the future be sufficient to serve as cautionary signals, a constant watchfulness should be observed by the etherizer, and every symptom carefully noted, especially all complications arising from bronchial secretion, pulse failure, or vomiting, and I am satisfied that not only should the patient be watched by a medical man during anæsthesia, but until reaction has been thoroughly established, and the ether effects entirely passed over; I have seen, as probably all surgeons have, ether poured upon the inhaler or napkin in great excess, so as to flow down on the patient's face and neck; the cloth thus becomes saturated, is not pervious to air, and the patient almost suffocates; such practice cannot but be severely condemned.

The assistant should devote his attention exclusively to the patient, while a few drachms at a time, with care, poured on the napkin will generally induce anæsthesia promptly; when the napkin becomes the least soiled, another should be substituted, and when complete anæsthesia has been induced, occasional inspirations of pure air should be allowed. Ether has been most successfully employed in the Pennsylvania Hospital for more than twenty-five years; it was introduced about 1850, and first used in a case of luxation of the femur, which resisted repeated attempts at reduction with the pulleys; it is needless to say that after etherization no difficulty was experienced, and from that time to the present this agent has been in daily use, and with the few exceptions herein noted has been the only anaesthetic employed.—*American Journal of the Medical Sciences*, October.

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## MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris, Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

Cutaneous Absorption of Iodine.—It appears from the observations by M. Jules Simon and M. Paul Reynard, that iodine may be absorbed by the cutaneous surface; but a large surface of application is needed before the iodine can be detected in the urine in a short time. In the experiments on children, the absorption in one of the cases was twice followed by iodism, and in half the cases there was albuminuria, sometimes of a marked character.—*L'Union Medicale*, 6th July. In the *Moniteur Thérapeutique*, 7th August, is a report of a clinical lecture by M. Simon on Iodine in the Therapeutics of Infancy. He insists that tincture of iodine should not be applied pure in tubercular children, but should be diluted with glycerine or some ointment. Iodide of potassium or iodide of iron should not be given to children under two years of age, except in cases of acute hereditary syphilis, when small doses may be given. Children after two years bear the salts, comparatively speaking, well. The following are the conclusions of M. F. Berg as to the pharmacology of iodine preparations:—1. The combination of iodine with albumen is very feeble, and can be broken up either by dialysis or coagulation. 2. The alkalies of albumen, when in true solution, are not saturated by free iodine; solutions of iodine that are free from salt or neutral in reaction immediately become acid on the addition of iodine, probably in consequence of the formation of hydriodic acid. 3. When the combination of iodine and albumen is broken down by the act of coagulation, the alkali set free from the albumen combines with the iodine. He finds from his experiments on the action of iodine that there is no exfoliation of the glands of the stomach in dogs after the toxic action of iodine. 2. Alkaline iodides in toxic doses cause serous, not bloody exudations, especially in the pleural cavities; free iodine, on the contrary, causes sero-sanguineous exudation and haematuria. 3. In animals poisoned with iodine, the arteries are not constricted.—*Centralblatt für die Chir.*, No. 4, 1876.

Bismuth.—Professor Cleland advocates the employment of liquor bismuthi as an application in hæmorrhoids and prolapsus, are particularly in those cases in which the mucous membrane is considerably involved.—*Practitioner*, January 1876.

Dr Martineau gives subnitrate of bismuth in the intestinal hæmorrhages which supervene in the course of typhoid fever. He administers fifteen grains of the powder every half-hour until the hæmorrhage ceases. In five cases so treated, the result was rapid and effectual.—*Lyon Med.*, 6th August 1876.

In the *Medical Times and Gazette* for 9th September, Mr I. H. Wathen calls attention to the recommendation of Dr Ferrier to administer bismuth in coryza. The treatment is not novel. M. Monneret, so far back as 1859, having recommended the subnitrate of bismuth in acute coryza, Dr Lobrier in the same year prescribed for chronic coryza a snuff consisting of four parts of subnitrate of bismuth, eight parts of liquorice powder, and thirty parts of iodide of sulphur. Of this ten or twelve pinches are to be taken daily. Dr Balmanno Squire suggests a solution of nitrate of bismuth in glycerine as a valuable preparation both for internal and external use. He is of opinion that as an application to the throat, larynx, vagina, uterus, and urethra, as well as to the skin, and no less as an internal remedy, glycerole of nitrate of bismuth will be found to open out a new field of therapeutics. For internal administration, the dose of the glycerole must be small and largely diluted, and for external use the strength of it should not exceed ten grains to the ounce.—*Pharmaceutical Journal*, 11th November 1876, and *Medical Times and Gazette*, 9th December.

It is the experience of Mr John Williams that solution of the nitrate of bismuth in glycerine is best effected in the cold. If much heat be employed in the preparation, the glycerole, when diluted, does not give a clear solution, at least after a few hours. The property of bearing dilution without producing turbidity appears to diminish by keeping. The diluted solution does not bear boiling, but when so treated deposits a basic salt, not afterwards soluble in water. Caustic potash or soda added to the glycerole diluted with water first causes a white precipitate, which is, however, perfectly soluble in excess of the alkali, a bright clear liquid being produced, which is mixable with water in all proportions, and might possibly be employed as a substitute for the *liquor bismuthi et ammoniæ citratis* of the Pharmacopœia. This alkaline solution of the bismuthia glycerole is precipitated white with alcohol, the precipitate not being again soluble in water. By boiling also a precipitate is produced. Ammonia cannot be substituted for potash in the above reaction.—*Pharmaceutical Journal*, 9th December 1876.

In a paper read before the Pharmaceutical Society of Great Britain, Mr S. C. Betty brought under notice a new soluble preparation, the oleate of bismuth, containing 20 per cent. of the base.

The oxide of bismuth (B. P.) is ground very fine, and the oleic acid is gradually incorporated with it. The mixture being placed in a suitable vessel is subjected to a temperature of nearly its boiling point, and is then allowed to digest with frequent agitation at a temperature of about 60° during four days, or until it solidifies. The result is pharmaceutically a plaster, chemically an oleate of bismuth. As to the utility of this preparation, Mr Betty states that it melts rapidly in contact with the skin, is bland to an excoriated surface, and penetrating by its liquidity. This product presents the bismuthic base in a condition most favourable for absorption, and it is to be hoped will prove a remedy, or at least an adjuvant, in cutaneous affections, of the exanthematous type.—*Pharmaceutical Journal*, 9th December.

JABORANDI.—In a leader in the *Pharmaceutical Journal* for 24th June 1876, the results of Mr Kingzett's researches relative to the chemistry of jaborandi are summarized. Mr Kingzett read his communication on the subject before the Chemical Society, 15th June. He adopts as the formula of the alkaloid obtained by him, $C_{23}H_{34}N_4O_4,4H_2O$. Hardy finds that the best method of obtaining pilocarpine is to make an infusion of the jaborandi leaves, evaporate to a syrupy consistence, mix with excess of magnesia, and evaporate to dryness. Treat this mixture with chloroform and then with water; the chloroform is then evaporated in a vacuum, the water also evaporates, and the pilocarpine is obtained as a syrupy mass, which is soluble in water and alcohol, and forms crystallizable salts with hydrochloric, nitric, and sulphuric acids. When jaborandi leaves are distilled with water they yield an essential oil of complex constitution. It is very analogous with the essential oil of citron, and does not appear to have any physiological action. For details, reference may be made to M. Hardy's paper in *L'Union Pharmaceutique*, vol. xvi. p. 365.

In the *Journal of Anatomy* for October, Mr Langley has a paper on the action of pilocarpine. He finds that in small doses, *i.e.*, up to about 30 mgr., it exerts an action in the salivary glands very similar to that produced by stimulation of the chorda tympani. It causes a rapid secretion and a considerable increase of blood-flow; both secretion and blood-flow gradually decline. Its effects are little, if at all, altered by section of the chorda tympani, or of the sympathetic nerve. Stimulation of the chorda tympani increases the pilocarpine effects. The secretion is stopped by injecting atropine, but a quantity of atropine, sufficient to paralyze the chorda tympani, does not prevent a relatively large quantity of pilocarpine from producing its ordinary effects. In fact, the secretion or absence of secretion is dependent on the relative quantity of the two poisons present, just as is the stand-still or beat of the heart. In larger doses, instead of causing a stronger flow of saliva, it causes none at all, and further prevents the chorda tympani from producing any secretion. It considerably diminishes the blood-flow through the gland, as well as the effect of the chorda tympani on the blood-flow. It does not, however, stop the sympathetic secretion. The action indeed is not very dissimilar to that of atropine; this agrees with its action on the vagus and inhibitory apparatus of the heart, where in large doses it prevents any inhibition of the heart from stimulation of the vagus, or of the junction of the sinus venosus, just as atropine does.

D. O. Kohler and Dr I. Loyka have lately been prosecuting some experimental researches regarding the physiological action of jaborandi. They injected into the veins of the animal a strong infusion of the leaves. Small doses caused a rapid elimination of blood-pressure and temporary increase in the frequency of the pulse. Larger doses have a more permanent action on the blood-pressure. After the primary increase of frequency, when the blood-pressure is restored the pulse falls, and, at the same time, the size of the pulse wave increases. Still larger doses cause great and permanent diminution of the blood-pressure and coincident retardation of the pulse. These effects the authors attribute to excitation of the terminations of the vagus in the heart, for they were produced after section of the vagus on both sides, while they were not produced when the vagus terminations in the heart had been previously paralyzed by atropine. These facts are in support of the view that jaborandi and atropine are antagonistic.—*Centralblatt fur die Med. Wissenschaften*, No. 31, 1876.

Stumpf has made many observations as to the action of jaborandi. With regard to the secretion of sweat, in 50 experiments only 2 gave negative results; in 44 cases in which the weight was estimated, the excretion through the skin and lungs ranged from 1470 to 13,425 grains, the mean being 7080 grains; whereas in an individual also under treatment, but without jaborandi, the mean loss in the same time was ascertained to be 1350 grains. The increased flow of saliva which supervened each time remained on the whole somewhat longer than the diaphoresis; the one had a mean duration of two hours and seven minutes, the other two hours and eighteen minutes. It yielded, in 39 observations, the average of 3870 grains. On analysis the organic constituents were found to be diminished, the salts in most cases increased. In about three-fourths of the cases the nasal excretion and tears were increased, and also the bronchial mucus. No qualitative change could be ascertained in the urine; whether quantitative alterations took place could not be determined definitely. The drug only occasionally reduced the temperature in febrile and non-febrile persons. The pulse was always accelerated. The respirations were often increased, but as often diminished. Of unpleasant consequences, nausea occurred in about half the cases; vomiting less often. Sleeplessness and headache came on in many cases. Strangury was observed only three times.—*Deutsch Arch. f. Klin. Med.* xvi., quoted in *Centralblatt f. d. Med. Wissen.*, No. 24, 1876, and in *Can. Med. and Surg. Journal*, August.

Pilicier states that he has found the diaphoretic action of jaborandi very moderate. The sweat was at first acid, then it became neutral, and often slightly alkaline. The sialogogue effect followed. The temperature sank in the course of two or three hours 0·3°–1·0° C. In most cases this fall was preceded for about half-an-hour by a rise. In guineapigs, dogs, and cats, jaborandi produces increased flow of saliva and tears, and excites diarrhœa, with increased peristaltic action of the intestines, also in the two last vomiting and copious urination. Strong doses injected into the veins caused death by dyspnœa in a few minutes. The antagonism between atropia and jaborandi was demonstrated satisfactorily in many experiments.—Quoted in *Centralblatt f. d. Med Wissen.*, No. 24, 1876, and in *Can. Med. and Surg. Journal*, August.

D. E. Bardenhever, in an inaugural dissertation published at Bonn, states that jaborandi exerts certain secondary actions, as an affection of the vision, myopia, haziness in the field of vision, and diplopia, and also disturbance of the functions of digestion from the large amount of saliva swallowed. It appears, too, to act powerfully on the sympathetic system; the saliva, for example, resembling, on account of its high specific gravity, the so-called sympathetic saliva. —*New Remedies*, September 1876.

From the experience of Dr Purjesz, he concludes that in skin diseases jaborandi seems to cause no change, and he regards it as an unsuitable drug in the treatment both of heart and kidney diseases attended with dropsy. He considers that the properties of jaborandi can only be duly estimated when its active principle shall have been successfully isolated, so as to avoid the injurious operation probably exerted by the other constituents.—*Deutsches Archiv für Klinische Medicin*, 26th May 1876.

The following experiments were performed by Dr Schwalm in the physiological laboratory of Giessen:—1. The chorda tympani was divided in a dog, and the superior cervical ganglion and adjoining portion of the sympathetic were excited. It was found that the flow of saliva caused by the injection of jaborandi continued, and that an injection of an additional dose of this drug caused an increase in the amount of saliva eliminated. 2. The blood flowing from an opened inferior maxillary vein in a dog under the influence of jaborandi was found to be bright red, was two or three times larger in quantity than natural, and in one instance pulsated. This augmented flow of blood from the veins was also observed after the chorda had been divided. 3. In rabbits the pill-like fæces were expelled suddenly, and in large quantity. When the abdominal walls were so divided that the intestines could be seen through them, it could be seen that immediately after the administration of jaborandi, both large and small intestines underwent violent peristaltic movement, which lasted for an hour or more—*Centralblatt f. d. Med. Wiss.*, No. 25, 1876.

It would appear from the writings of Stumpf that in Germany an unfavourable opinion was at first entertained relative to the action of jaborandi, but Riegel Merkel, Penzoldt, Rosenbach, and others, agreed as to the energetic diaphoretic action of this drug. Lohrisch considered this action to be uncertain, and salivation the only constant phenomenon. Stumpf has had experience of this drug in 54 cases, 14 of which were fever cases. He says that the most striking and constant symptom in the action of jaborandi is the hypersecretion of certain glands of the skin, and of particular mucous membranes, and the sweat glands are chiefly called into heightened activity. The loss of fluid by the skin and lungs is estimated at about five times the normal amount. The salivary secretion is also notably increased, and likewise the tears and the nasal secretion are augmented. Its effect on the secretion of urine is not constant, but its amount is diminished. In patients not suffering from fever it caused in only a very few cases some lowering of the bodily temperature; but of ten fever cases, the temperature fell in seven. Stumpf has observed good effects from jaborandi in a case of general dropsy resulting from chronic pericarditis. It may be recommended in rheumatic affections, especially in the sequelæ of acute and chronic rheumatism of the joints.—Quoted from *Deutsches Archiv Klin. Med.* in the *Brit. and For. Med.-Chir. Review*, April 1876.

Dr R. S. Peart states that jaborandi, in doses of five grains of the powder infused and taken three times a day, had a very decided effect in increasing the secretion of milk in several cases in which there was hardly any flow. The resulting milk agrees quite well with the infant. A good and liberal diet should of course be given to the mother.—*Brit. Med. Journal*, 18th November 1876.

The following are the conclusions arrived at by Dr Grassel of Montpellier as regards the use of jaborandi in pleuritic effusions:—1. Jaborandi is very useful in pleuritic effusions, whatever may be their date and the amount of fluid. 2. It usually causes a rapid disappearance of the fluid and the production of the pleural friction sounds. 3. Its effects are of short duration, so that the fluid is often rapidly reproduced, in which case the use of the drug must be persevered in. 4. When the fluid has disappeared, and the pleural friction sounds are heard, the jaborandi is no longer efficacious; and to complete the cure recourse should be had to a tonic treatment and to local applications.—*Jl. de Thérapeutique*, 10th April 1876.

Dr Bompart relates a case of commencing tetanus, the cure of which he attributes to jaborandi. Four grammes of the pounded leaves were made into an infusion. The same dose was given daily from 23d Jan. to 3d March, when all symptoms of tetanus had disappeared. The jaborandi produced very abundant salivation.—*L'Union Médicale*, 7th March 1876.

Professor Ringer and M. J. S. Bury have an interesting paper in the *Practitioner* for December on the effect of pilocarpin in two cases of unilateral sweating. One case was that of a woman aged 46, who, for nine years, was the subject of very pronounced unilateral sweating, the perspiration being profusely secreted on the left side. Injection into the left arm of one-third grain of pilocarpin brought on sweating, which was most abundant, on the right side. After a second injection next day, this time on the right arm, the sweating was most profuse on the left side. On the following day an injection into the skin of the right arm was followed by sweating at first on the left side, but after five minutes the sweating became equal on both sides. After several subsequent injections the unilateral sweating was completely cured. The other case was that of a man aged 55 years, and in this case the effect of the drug on the perspiration was less marked; the first injection, however, greatly lessened it. After twelve days, notwithstanding repeated injections, the unilateral sweating returned, and thenceforward the pilocarpin completely failed to control the sweating.

Dr Dowse lately read a paper before the Harveian Society, in which he gives a comprehensive summary of what is at present known of the physiological action and therapeutic value of jaborandi. The paper is published in the *Medical Press and Circular* for 27th Nov. 1876.

QUININE.—*Physiological Action.*—According to Jerusalinsky quinine in small and medium doses (1 to 5 grains) causes an increase in the frequency of the pulse. The blood-pressure, on the contrary, has a tendency to fall, except immediately after the injection, when there is generally a period of increase. Large doses (20 to 25 grains) cause the pressure to sink rapidly, and generally at once. The increase in the frequency of the pulse is due to depression or paralysis of the inhibitory, and excitation of the excito-motor nervous system. The period of increase in the blood-pressure Jerusalinsky attributes to paralysis of the inhibitory and excitation of the vaso-motor apparatus. Small doses act as a tonic to the heart, and large doses paralyze that organ. Small doses increase the rapidity of the respiration, large doses render the respiration slower and irregular. The action on the temperature is inconstant; in most cases it falls, but sometimes a rise was observed. The rapidity of the flow of blood is retarded by quinine to one-half its original rate, due to paralysis of the vaso-motor centres. It arrests the amæboid movements of the white corpuscles, and diminishes their emigration from the vessels and their number. With Manassein, he found that the red corpuscles became larger under the influence of quinine. He also confirms the observation of Mosler, that quinine causes diminution in the size of the spleen, and renders it tough and granular on the surface, and brighter in colour. These results are attributable primarily to the effect of the peripheral nervous and muscular elements of the spleen; and, secondarily, on the splanchnic and central nervous system.—*Centralblatt f. d. Med. Wiss.*, No. 26, 1876.

*Elimination of Quinine.*—MM. Albertoni and Ciotti find that quinine given internally passes into the bile, and may be demonstrated in that fluid two or three hours after its administration. If a ligature be placed on the portal vein, or if the quinine be administered hypodermically or injected into the arterial system, it is eliminated by the urine and does not enter the bile.—*Bull. de Thérapeutique*, t. xc., page 360.

*Quinine as an Ecbolic.*—Dr Chiarleoni draws the following conclusions from his experiments conducted in the Maternité of Milan: 1. Sulphate of quinine is incapable of abbreviating the pause between one uterine contraction and another, and hence cannot augment the number of them. 2. The duration of the contractions before and during the action of quinine does not vary sensibly; the use of this drug would be disadvantageous, perhaps, from the sedative action exerted by it on the nervous system. 3. Quinine given near the time of parturition in doses of 0·50, 0·70, 1·00, and 1·50 grammes has no sensible action on the condition of the uterus. 4. Quinine does not seem to possess any power to augment the intensity of the uterine contractions. 5. Contractions, whether regular or irregular, are not altered in character by quinine.—*Gaz. Med. Ital. Lombard.*

*Quinine in Gonorrhœa.*—Radha Nauth Roy extols the efficacy of injections of quinine in gonorrhœa. He states that he has invariably found the disease to yield, both in its acute and chronic stage, under the influence of this treatment. The quinine acts as a tonic and astringent to the mucous membrane of the urethra. He has also used this drug in some cases of cystitis with benefit. His formula is two grains of sulphate of quinine, dissolved in an ounce of rose-water by the aid of 8 or 10 minims of dilute sulphuric acid.—*Indian Med. Gazette*, May 1876.

*Quinine in Sunstroke.*—In the *Lancet*, 29th July, is recorded a case of sunstroke, under the care of Dr E. L. Fox, in the Bristol Royal Infirmary, successfully treated by subcutaneous injections of quinine. Twenty grains of quinine were dissolved in a drachm of sulphuric acid and half an ounce of distilled water; of this one drachm was injected under the skin at various places in quantities of six minims at a time.

In the *Canada Medical and Surgical Journal* for September, a case of sunstroke, treated by Dr Drake, is reported by Dr Cameron of Montreal. Thirty grains of quinine in three doses, at intervals of half-an-hour, were given. Two hours after the first dose the skin became moist, and was soon covered with profuse perspiration; urine passed freely. Six hours after the administration of the quinine the temperature had fallen from 107·8° to 103°, and fourteen hours after to 100·4°. On the following day five grains of quinine were given every four hours; four doses were given. All bad symptoms rapidly disappeared, and in three and a half days the patient was discharged cured.

*Eruptions produced by Quinine.*—Dumas relates the case of a woman suffering from facial neuralgia, and in whom 4½ grains of sulphate of quinine produced a paroxysm of asthma, coryza, fever, violent itchiness over the whole cutaneous surface, and an eruption of urticaria; red spots like the scarlatinal rash and papules also formed in various parts. On four occasions the administration of quinine caused similar effects in this patient.—*Jl. de Thérapeutique*, No. 16, 1876.

*Quinine subcutaneously.*—In a letter to the *Lancet*, 22d July, Dr A. D. Campbell states that he finds a single injection of five grains is sufficient to prevent a recurrence of the febrile paroxysm, a second or third injection being rarely required. He commends this treatment in malarious fevers for its economy, efficacy, and rapidity of effect. He finds it equally efficacious in cases of intermittent neuralgia. In two cases only, out of about six hundred, did any evil local effects ensue. He uses neutral sulphate, which does not require an acid for its solution. He suggests that the solution should be carefully filtered; that the syringe should be perfectly clean; that the needle should be got well into loose cellular tissue; and that, on the withdrawal of the syringe, the fluid should be dispersed by careful kneading.

*Vehicle for Quinine.*—Dr Isaac Smelt, jun., finds that one ounce of sweet spirit of nitre will dissolve about two drachms of sulphate of quinine, giving a transparent solution.—*New York Medical Journal*, August.

*Bromhydrate of Quinine.* The greater solubility of this salt, and the greater percentage of alkaloid which it contains, renders it suitable for hypodermic injections. In large doses it gives rise to headache, ringing in ears, deafness, muscular weakness, and drowsiness. For hypodermic use, a solution in the proportion of one to ten in alcoholized water is recommended.—*Journal de Thérapeutique.* In the American Journal of Pharmacy, Mr W. Silver Thomson gives a formula for making hydrobromate of quinine for hypodermic use, the commercial bromide of quinine not being sufficiently soluble for this purpose. Dissolve 96 grains of sulphate of quinine in 4 ounces of distilled water by the aid of just sufficient sulphuric acid. Mix 150 minims of water of ammonia with 4 ounces of distilled water, and add it to the solution of quinine, constantly stirring. Transfer to a muslin filter, and test the filtrate of solution of sulphate of ammonia for quinine. If the presence of the latter be indicated, add a little more water of ammonia to the whole solution, and return it to the filter. Wash the quinine with water, and transfer the moist magma to a tarred capsule, and add gradually just sufficient hydrobromic acid to dissolve it. Evaporate on a water-bath until a transparent mass is obtained; and, lastly, dissolve in a sufficient quantity of distilled water, so that each drachm shall contain 12 grains.

*Salicylate of Quinine.*—In the November number of this Journal is a brief but interesting paper by Mr J. Graham Brown on Salicylate of Quinine as an Antipyretic. The paper is illustrated with temperature charts, which show clearly the great power of the salt in this respect.

*Quinetum.*—In a communication to the *Medical Times and Gazette*, 21st October, Dr Thorowgood calls attention to the value of quinetum as prepared by M. T. Whiffen of Battersea, in cases where sulphate of quinine does not agree. Quinetum is a pale brown amorphous powder, consisting of a mixture of the alkaloids of the East India red bark. Dr Thorowgood has given the crystallizable sulphate of quinetum in seven cases, and in none was there any unpleasant result. It is cheaper than sulphate of quinine, and is hence adapted for hospital purposes.

*Relative Value of Cinchonidia and Quinia.*—The Medical Board of Bellevue Hospital, New York, has recently caused experiments to be made as to the relative merits of the sulphates of quinia and cinchonidia. As a result of the investigations, it has been found that cinchonidia is a valuable tonic and anti-febrile remedy, but is inadmissible in severe surgical injuries, especially of the genito-urinary system, and in cases of shock, in which it is apt to produce emesis and severe congestive symptoms; in such cases quinine had to be resorted to. In purely medical cases it was found to answer well—not so promptly, however, as quinine, and requiring a larger dose. The cinchonidia is much more inexpensive than the quinine salt.—*London Medical Record*, 16th October 1876.

# Part Fifth.

## MEDICAL NEWS.

Royal Medical Society.—The following gentlemen have been elected office-bearers:—*Presidents*—J. Graham Brown, M.B., C.M.; Joshua J. Cox, M.B., L.R.C.P. and S.; Robert Kirk, M.B., C.M., L.R.C.P. and S.; James Baker, M.B., C.M., L.R.C.S. *Honorary Secretaries*—J. Milne Chapman; Robert Roxburgh, M.B., C.M., L.R.C.S. *Curator of Library*—Lionel Druitt, M.R.C.S. Eng. *Curator of Museum*—Chas. W. Cathcart, M.A. The Society is in a very satisfactory state at present, 50 new members having already signed the roll.

## OBITUARY.

### JOHN GAIRDNER, M.D., F.R.C.S. Ed.

The Edinburgh newspapers have, during the past month, recorded the death of this estimable and venerable gentleman, and have borne testimony to his worth by obituary notices of him as an Edinburgh citizen. To us, as medical journalists, his death forms an object of much interest, as it is the breaking of one of the few remaining links in the chain which connects the present with a past generation of medical men.

Dr Gairdner, the oldest son of Captain Robert Gairdner, of the Hon. East India Company's Service, was born 18th September 1790, at his father's beautiful residence, Mount Charles, on the "banks an' braes o' bonnie Doon," and was thus in his 87th year at the time of his death. Captain Gairdner was killed by an accident when the subject of the present notice was only five years old, and consequently John Gairdner and his four brothers were brought by their mother for education, first to the, then as well as now, excellent Ayr Academy, and subsequently to Edinburgh. John, having embraced the medical profession, took the diploma of the College of Surgeons of Edinburgh, and his degree from the University in 1811, and thereafter went to London and studied under Charles Bell. Well equipped in the knowledge of his profession, he returned to Edinburgh in 1813, in which year he engaged in practice in partnership with Dr Farquharson. In the same year he was admitted to the Fellowship of the Royal College of Surgeons, and thus was a Fellow of the Incorporation at the time when many men who have now grown gray in the practice of their profession were sucklings. Dr Gairdner's acuteness of mind, and earnestness in the discharge of every duty which fell to his lot, prevented him from being a silent or indifferent member of the College. He early took a part in their proceedings, and so soon were his talents and acquirements recognised by the other Fellows, that in 1817, only four years after his admission to the Fellowship, he was elected an Examiner. He subsequently filled the Presidential Chair from 1830 to 1832, and for many years was Treasurer of the College, in which capacity it was part of his duty to scrutinize the class certificates of those who presented themselves as candidates for the diploma of the College. In every position which he filled in the College of Surgeons, with which Body his whole professional life was identified, Dr Gairdner maintained his character of a man thoroughly imbued with a sense of his responsibilities. As an active Fellow of the College, and consequently a party to medical politics, he went heart and soul into every movement which he held to be essential to the maintenance and elevation of the character of the profession. He had no fear that the demanding of higher qualifications from candidates would impoverish the College by lessening the number of its licentiates, and he therefore went in for increased preliminary education and extended professional curriculum. He felt that if diplomas were granted, after full education and searching examination of aspirants to the medical profession, there was neither justice nor common sense in the laws which then excluded a Scottish licentiate from practising all the departments of his profession in England; and therefore, along with such men as William Wood, a great Conservative in imperial, but a thoroughgoing Liberal in medical politics, and David Maclagan, like Gairdner, a Liberal in both respects, he had no small share in those movements which brought to an end the monopoly of the Apothecaries Act of 1815, and led to the Act under which the medical profession in Britain now exists. Another measure which was promoted by these gentlemen and those who acted with them, in the carrying out of which they were much aided by the late Adam Black, afterwards M.P. for the city, was that which enabled students in the University to take a certain proportion of their medical education in the Extra-academical Schools, so that if a University Professor from any cause became inefficient, the students might avail themselves of the instructions of an extra-mural teacher. This measure, as was predicted by its authors, has not only not injured but has substantially benefited the University as a Medical School.

As an Examiner Dr Gairdner was rigid, but not over-exacting. Candidates who knew him not to have been an *ex professo* anatomist, were surprised at his anatomical knowledge, but did not know how closely he had worked in Windmill Street with Charles Bell and John Shaw. When he was questioning them in chemistry, they were unaware of the fact that he was a man who read up to his time, and knew what was going on in the world of practical science. He was at one time—all honour to him for it—one of the *bêtes noires* of ill-prepared candidates. As President of the College he commanded the respect and obedience of the Fellows; and the College has testified its regard for him by hanging his portrait on its walls. It must be remembered that in those days, antecedent to parliamentary and municipal reform, the head of the College of Surgeons was not merely the President of a professional body, but was, *ex officio*, a member of the Town-council as Deacon of the Surgeons, who constituted one of the incorporated trades of Edinburgh. In this way Dr Gairdner was led to take an active part in municipal matters, and proved himself to be an eminently useful and public-spirited citizen. He obtained prodigious popularity in Edinburgh by seconding the nomination of Lord Advocate Jeffrey as Member for the City against one of the powerful family of Dundas, the election being at that time vested in the thirty-three persons who constituted the Town-council. His services as Treasurer ought never to be forgot by the College of Surgeons of Edinburgh. He had charge of the finances during a long and dismal period, when the College was financially in very low water. This corporate impecuniosity did not arise from corporation wastefulness, still less from personal appropriation of the finances by the Fellows, but from the free outlay of its funds in the building of the present hall, and the establishment of that noble museum of healthy and morbid anatomy, which it has at all times so freely opened to the profession and the public. John Gairdner could relax the purse-strings of the College when anything was required in the way of expenditure which was necessary for the dignity of the College or the welfare of the profession; but when this did not seem to him to be the case, he could hold them tight with the grasp of a Harpagon. It is due to his memory, as it is to the credit of the few of his contemporaries, such as James Combe and William Brown, who survive, to record that, in order to save the finances of the College when these were low, these men for some time disinterestedly discharged the duty of Examiners without either fee or reward, except the thanks of the College and the approbation of their own consciences.

The leading characteristics of Dr Gairdner's personal character were his unbending honesty of purpose and inflexible integrity. No man ever better, by his own life and conduct, exemplified the *justum et tenacem propositi virum.* He was essentially straightforward, said what he meant, and meant what he said. Any one might differ from him, for he had his own notions on things, but no one could doubt his sincerity. He had always the courage of his opinions, and he had a shrewd quiet way of showing reasons for them. He was a well-read, accomplished gentleman, a good Latinist, and well versed in the literature of his own country. As a "leal" Ayrshire man, and native of the land of Burns, he was an enthusiastic admirer of the gifted peasant poet. He had a genial appreciation of anything in prose or poetry which was humorous or witty, and many of his juniors—for few of those near his age and none senior to him survive in the profession in Edinburgh—can remember the smile of mouth and twinkle of eye with which he would quote long passages from the "Fudge Family," the satire of which consorted with the ideas of a man who had, as one of the steady old Whigs, lived through the regency and reign of George the Fourth.

Dr Gairdner was not, like his distinguished son, the Professor of Medicine in Glasgow, a large contributor to the literature of his profession. His printed writings partook a good deal of the antiquarian character, *e.g.*, his notice of the deaths from smallpox in the Royal Families of Europe before vaccination, and his very curious and exhaustive history of the Royal College, with which he was so long connected. He was, however, one of those men who, without much to show in print, made his impression on the medical profession. He aided in no small degree by his work, along with those who have been named above, in promoting those desirable reforms of which the profession in Britain is now reaping the benefit; and for this, as well as for his personal character, he is to be held in grateful and affectionate remembrance.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*On some of the Relations of the Fœtal Head to Rupture of the Perineum and Injuries of the External Genital Organs.* By J. MATTHEWS DUNCAN, M.D.

*(Read before the Obstetrical Society of Edinburgh, 10th January.)*

IN a former paper on Perineal Rupture, I discussed[1] some of these relations, but I now wish to enter upon the subject more fully.

While the passage of the fœtal head through an ordinary contracted pelvic brim has been carefully studied, and many of the mutual influences well made out, the same cannot be said of the passage of the fœtal head through the vaginal and then through the vulvar orifice. In important respects the former passage is an easier and simpler subject of study than the latter, and there is in consequence a contrast between them. The passage through the brim is the forcing of the globose head through a passage contracted at one part. The passage through the vaginal and vulvar orifices is through an obstruction which is circular, or nearly so, and acts at every part. The passage through the brim is therefore a matter affecting one diameter of the fœtal head, or a single series of diameters of nearly the same name, extending in a nearly vertical line from above downwards on the fœtal head. The passage through the vaginal and vulvar orifices affects a circumference or circumferences of the head, embraced, as it is, all around by the opposing undilatable margins of the orifices. In the passage through the brim, the head has to suffer and be moulded, while the resisting parts are practically unyielding, and may be regarded as unaffected. In the passage through the vaginal and vulvar orifices the head has to suffer to some extent, and be moulded slightly, but the resisting parts are much more moulded, and must yield or be lacerated.

The passage of the fœtal head through a generally contracted pelvic brim has more likeness to the passage through the vaginal and vulvar orifices than has the passage through an ordinary contraction affecting the conjugate diameter chiefly. In both of the former cases, the whole circle of the aperture, or nearly the whole, resists the advance of the head. In both, and for nearly the same reasons, the head advances or is forced onwards in that way in which it passes most easily; that is, with one end of the long diameter of the oval head coming first; or, with the posterior triangular fontanelle, or some part near it, forming the presenting point.

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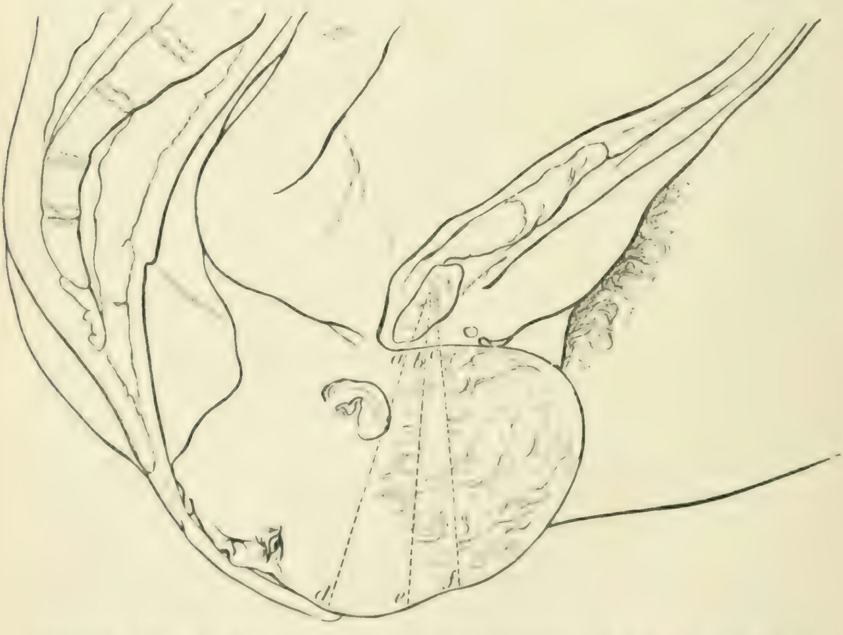
[1] Edinburgh Medical Journal, March 1876, p. 828.

The fœtal head does not come through the vaginal and vulvar orifices simply or exactly in the line of direction of its long axis. That is prevented by the connexion with it of the neck, which forbids the amount of flexion-like movement that might occur if the neck and body were imagined to be absent. Besides, in consequence of the presence of the neck of the fœtus, and from the construction of the passage, the head follows the line of the so-called circle of Carus; not a straight course even at its moment of emergence from the genital canal. With its suboccipital region pressed against the middle of the pelvic arch, and advancing slowly, wheeling around a point somewhere about the centre of the symphysis pubis, the region of its vertex is advancing rapidly in a larger circle, wheeling around the same point, and distending the perineum to its utmost.

This is well shown by the accompanying woodcut, which is



adapted for the illustration from the nineteenth plate in Smellie's Anatomical Tables. In this drawing, *a b c* represents the suboccipital region, or a line joining the occipital protuberance and the upper region of the neck or nucha. *e f* represents the suboccipito-vertical diameter: it passes through the plane of the parietal tuberosities and reaches the sagittal suture about ¾ inch behind the centre of the anterior fontenelle: it measures about 3½ inches; and its corresponding circumference passing over the parietal tuberosities measures about 11¼ inches. *b c* is the suboccipito-bregmatic diameter: it passes from the suboccipital region at a point a little nearer the nucha than the former, and reaches the middle of the anterior fontenelle in a line joining the limbs of the coronal suture: it measures about 4 inches, and its corresponding circumference measures about 12½ inches. *a d* is the suboccipito-frontal diameter: it passes from the suboccipital region at a point a little nearer the nucha than the former, and reaches the most prominent part of the forehead at the frontal suture, about an inch in front of the centre of the anterior fontenelle or the middle point of a line joining the limbs of the coronal suture; it measures 4¼ inches, and its corresponding circumference is about 12¾ inches. These various figures are not taken from a sufficiently large number of newly-born children to give them value as averages, but their value, when used in comparison one with another, is evident. They may be given in a tabular form, thus—

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| *e f* | suboccipito-vertical | diameter | 3½ | inches, | in circumference | 11¼ | inches. |
| *b c* | suboccipito-bregmatic | ,, | 4 | ,, | ,, | 12½ | ,, |
| *a d* | suboccipito-frontal | ,, | 4¼ | ,, | ,, | 12¾ | ,, |

Every practitioner supporting the perineum during the birth of the head must frequently have observed that it has retained its entirety until the projecting forehead came on, and this lacerated it. Now, this is easily explained by the above table of measurements, for it is not till the suboccipito-frontal diameter is passing that the perineum is stretched to the utmost. The suboccipito-vertical and suboccipito-bregmatic diameters may both pass over the perineum without tearing it, and then the large suboccipito-frontal may lacerate it on account of its greater dimensions. No doubt the suboccipito-bregmatic or the suboccipito-vertical diameter may tear the perineum, or even an earlier passing part; and in this case the tear may be extended and extended, as each larger part follows, till the largest suboccipito-frontal emerges, completing the injury done.

The order in which all the lacerations of the external genital organs take place I cannot decide, but the following statements are either self-evident or nearly certain.

Before the suboccipito-vertical diameter has passed the vaginal opening, or even when only a caput succedaneum protrudes, the vaginal orifice is lacerated posteriorly. The laceration increases as the head advances.

While the parietal protuberances pass through the vaginal and vulvar orifices, they may lacerate one or both sides of the vaginal orifice or of the vulvar orifice.

The vestibular lacerations take place before the suboccipito-frontal diameter has passed the vulvar orifice. Their occurrence helps to save the perineum from laceration.

A laceration of the fourchette may take place early in the passage of the head.

An extensive laceration of the perineum (say three-fourths of an inch or more) obviates tension of the vaginal and vulvar orifices, and therefore prevents further laceration after its occurrence.

**The perineum is the part last lacerated**

Cases of ordinary or characteristic central perineal rupture, in which the head advances in its natural way, and is born through the vaginal and vulvar orifices, and, still better, cases of incomplete central rupture, such as I have recorded in a former paper, require special consideration. They show nearly with certainty that the anterior part of the perineum is in some cases, including themselves, considerably more extensible than its middle or posterior part; for in them the anterior part (or a portion of it—it may not be the most anterior part or fourchette) remains entire, while the suboccipito-frontal diameter passes over it, and after the middle of the perineum has given way under the distension by the same diameter of the fœtal head.

In the cases of complete central rupture which I have seen there were two perineal ruptures; first, of course, the central rupture; and, second, an ordinary rupture of the fourchette or anterior part of the perineum. Between them, in the most characteristic cases, but not in every instance, there is an entire strap or bridge of skin unhurt. No doubt the unhurt bridge or isthmus of skin may, in some cases, be absent, and the two distinct ruptures be confused in one. Whether this be the case or not, it is to be remarked that both tears may go on simultaneously, and so rapidly that the observer may be unable to distinguish their separate occurrence. While the suboccipito-vertical or even a smaller circumference is distending, and, it may be, tearing the vaginal and vulvar orifices, the hitherto entire middle of the perineum may be at the same time suffering central rupture by the great advancing suboccipito-frontal circumference. The middle of the perineum may have endured dilatation by the suboccipito-vertical and suboccipito-bregmatic circumferences which have advanced to lacerate the vaginal and vulvar orifices, but cannot endure further distension by the suboccipito-frontal, which therefore tears it, and may yet pass over some of the anterior part of the perineum without destroying such anterior part.

Having discussed the injuries of the perineum produced by the passing fœtal head, we have now to devote attention to the injuries of the fœtal head, caused by the resisting perineum; for it may be held as quite certain that every obstacle or modification of resistance to the advancing head leaves its mark upon it. In the case of difficult passage through the pelvic brim, the changes or injuries are often well marked and have been carefully studied, with great advantage to the progress of obstetrical science. But the vaginal and vulvar orifices and the perineum also do in like manner produce their peculiar capita succedanea, equitations of bones and shears; and they demand more careful study than has yet been accorded to them. In the meantime I can only enter upon some general views.

The caput succedaneum of the vulvar orifice, or of the end of the second stage of labour, I have already elsewhere described.[1] That the perineum and vaginal and vulvar orifices may by their undilatability lead to equitation of bones there is no doubt, but I have not studied the subject so as to enable me to describe any peculiarity in such overlapping. In regard to the shear produced by the forcing of the head over a perineum, and through a vaginal and vulvar orifice difficult of dilatation, it may be stated that the lateral or antero-posterior part of the ordinary shear[2] is in great measure due to the resistance of these parts.

The mortality of male children in birth, and shortly after it, is greater than that of females. Males more frequently demand artificial assistance in delivery than females. There is no doubt that both these evil conditions are in part owing to the greater size of the male head, and the consequent need of more dilatability and more actual distension of the perineum and vaginal and vulvar orifices than is required by females, circumstances which imply greater injury of the fœtal heads of males.

It has been shown that the part of the fœtal head which most distends the perineum and vaginal and vulvar orifices is that generally called the suboccipito-bregmatic diameter or circumference (including the suboccipito-vertical, suboccipito-bregmatic, and suboccipito-frontal diameters or circumferences of this paper). Hence it should be expected that the suboccipito-bregmatic diameter should show evidence of special compression, and this has been admirably done by Budin.[3] His measurements were made to show the amount of resilition gradually effected within about two days after delivery, giving thus a satisfactory indication of the amount of compression during birth. "The augmentation (says he) of the suboccipito-bregmatic diameter has been constant (in all his fifty-two cases measured); this augmentation has been almost always very considerable, and has amounted even to twelve mms. The suboccipito-bregmatic diameter is, in fact, that which increases the most after delivery."

In accordance with what has been stated in this paper regarding the suboccipito-frontal diameter or circumference I have no special measurements to adduce in evidence, but I daresay every one will be prepared to believe in its particular diminution during labour, for the lowness of the forehead in a much-compressed head is well known to accoucheurs and is often a horrid sight to the parents ignorant of its temporary character. But in regard to the suboccipito-vertical diameter or circumference, I have valuable measurements by Budin to adduce, whose evidence quite supports the line of demonstration pursued in this paper. As already stated, the parietal protuberances are embraced in the suboccipito-vertical circumference, and Budin's measurements indicate the amount of diminution of the biparietal diameter, which is one measure of the suboccipito-vertical circumference. Budin describes, and his measurements show the bitemporal diameter, which corresponds to the suboccipito-bregmatic proper, more diminished during labour than the biparietal. Or, to use his own words, "The biparietal diameter also augments after delivery, but this augmentation is not constant; it is by much the least considerable: in fifty-two cases, two times only has it amounted to five mms., once to six, and once to seven mms."

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[1] Contributions to the Mechanism of Natural and Morbid Parturition, etc., 1875, p. 235.

[2] *Ibid.*, p. 216.

[3] De la Tête du Fœtus au point de vue de l'Obstétrique. Paris, 1876, p. 67.

If the argument of this paper is correct, then there should be a great difference between the fœtal heads of primiparæ and those of multiparæ. The greater injuries of the perineum and external genital organs in primiparæ than in multiparæ should be correspondingly reflected in the heads of the fœtuses born in primiparæ and in multiparæ. And so it is. The mortality in birth, and shortly after it, of children born of primiparæ, is greater than of children born of multiparæ. The heads of the children of primiparæ are more deformed by labour than those of multiparæ. Budin, our best authority on this point, writes as follows:[1]—"That this resistance of the soft parts—and Professor Depaul, following M. Dubois, insists strongly on this point—that this resistance of the soft parts causes, during labour in primiparæ, a longer duration of the period of expulsion, is a fact about which there can be no doubt. From it there results also a more marked deformation of the head. The figures which we have given, and which show considerable modifications in the form of the cranium, are all the consequence of deliveries in primiparæ. In multiparæ, on the contrary, the deformations are much less remarkable, and often are almost absent."

Here I must draw to a close, with the remark, that the subject is well worthy of further elucidation and description. Of two sets of facts I feel special want, namely, a statement of the actually observed injuries produced by the birth of males, as compared with those produced by the birth of females; and a statement showing comparatively the injuries produced in head last cases and head first cases. The matter is one not of so easy solution as it may at first sight seem, for mere great number of injuries may be made up for by severity in the smaller number. One serious laceration may be more important than several small ones. Besides, one deep laceration may act as a preventive of several smaller.

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[1] *Ibid.*, p. 70.

## ARTICLE II.—*Notes on Surgical Practice among the Natives in Shanghai.* By EDWARD HENDERSON, M.D. Edin., Municipal Surgeon and Health Officer, Shanghai. No. II.

(*Continued from page 414.*)

*Calculus.*—Stone in the bladder is a disease practically unknown in the north of China. In the south—in the neighbourhood of Canton at least—it is of so common occurrence as to have furnished a hospital (the Medical Missionary Society's Hospital in Canton) with no fewer than fifty-one cases of lithotomy and lithotrity during the past year (1875). Once only, in March 1872 have I been consulted in Shanghai in a case of the kind; and my patient, a Eurasian child, aged 4, had been born in Macao, and lived there for nearly three years. A year or two ago, I heard of another case in an adult Chinaman; but I do not know the full particulars of it. The stone was, I believe, small, and removed by the lithotrite. Rarely, however, as bladder-stone is met with in the north, it has been my fortune to encounter in Shanghai a case of urethral calculus of so remarkable a kind, that I am inclined to believe it to be at present without a recorded parallel. The particulars of this curious case are as follows:—

CASE I.—In January 1872, a tolerably healthy-looking Chinaman, aged 20, consulted me at the American Episcopal Native Hospital, on account of a tumour of the penis, which caused him considerable inconvenience from its size and weight, and from the obstruction that it caused to the flow of the urine. The organ had a natural appearance as far as the base of the glans, but beyond that point had expanded into a globular mass, having, about its centre, a circumference of over twelve inches. This expansion was perfectly smooth, and covered by very tightly stretched, parchment-looking skin—evidently the expanded prepuce—in which no vessels were visible. In form, the tumour was, strictly speaking, egg-shaped, with the large end of the ovoid directed towards the penis, being also somewhat compressed from before backwards. It was of stony hardness and great weight. In the centre of the upper (anterior) surface, there was a circular opening—a shallow ulcer—with somewhat irregular, partially cicatrized, callous edges. A tube passed into this opening encountered at once what seemed to be a solid stone, covered by a thin crust of urinary salts. There was no other opening in the tumour, and this was the aperture through which the urine escaped.

The patient said that the growth first attracted attention when he was four years of age, and that it had been slowly increasing in size as far back as he could remember. He had carried it for many years suspended to his waist-belt by a cloth sling. He passed water without noticeable straining, the stream trickling through the opening already described.

I removed the mass in its entirety, and the lad was discharged from the hospital a few weeks afterwards, relieved of the encumbrance, and able to pass water in a full stream from the stump.

The tumour weighed after removal a pound and a half. On section, it was found to consist of an extraordinary number of small stones, enclosed in a thin shell formed of the expanded and attenuated tissues of the glans and prepuce. When first exposed, the stones were seen to be cemented together by a scanty deposit of mucus mixed with urinary salts; and it was easy to understand how, when enclosed in this tightly-fitting capsule, they gave to the hand perfectly the impression of a single stone of large dimensions. The stones were white in colour, tolerably hard, and perfectly smooth, varying in dimensions from the size of a pin's head to that of a small bean. The smaller, and by far the most numerous, were quadrilateral and laterally compressed, with a distinct depression on each side; those of the larger size were of less regular and more rounded form. Some were cylindrical, varying in thickness and ranging from a quarter to half an inch in length. These had short pieces of straw or bamboo for nuclei—pushed in, doubtless, from time to time by the patient himself, to clear the passage for the escape of the urine. In half a drachm's weight, taken at random from the heap, I counted 570 distinct calculi. In the accompanying engravings, Fig. 1 gives the external appearance of the tumour, the two sides of the divided capsule being held together by stitches. The lines *a a* show the connexion with the penis, and *b* is the orifice through which the urine escaped. Fig. 2 shows the interior of half of the shell, to which some small stones still adhere, and on which numerous indentations mark the former position of others. Fig. 3 is a glass jar containing the stones, which have been removed from their capsule and washed. This jar is about seven inches high, and has a diameter of about three inches. Several of the cylindrical calculi described above appear lying against the side of the glass. The urethra opened among the stones on the side furthest from the aperture through which the urine was discharged externally (see Fig. 2, *a*).

Besides this case of urethral stone, I have operated on two cases of preputial calculus, of both of which I have preserved notes, partly on account of the rarity of stone cases in this part of China, and partly on account of certain peculiarities in the history of each.

CASE II.—In November of 1874, an adult, and apparently healthy, coolie presented himself at the American Mission Hospital, complaining of difficulty in passing water. On examination, I found a congenital phymosis, the passage through which was so narrow as with difficulty to admit a common probe. Several hard bodies could be felt lying loose in the bag formed by the redundant foreskin. I performed a circumcision in the usual way, and removed twelve stones, about the size of peas, from between the

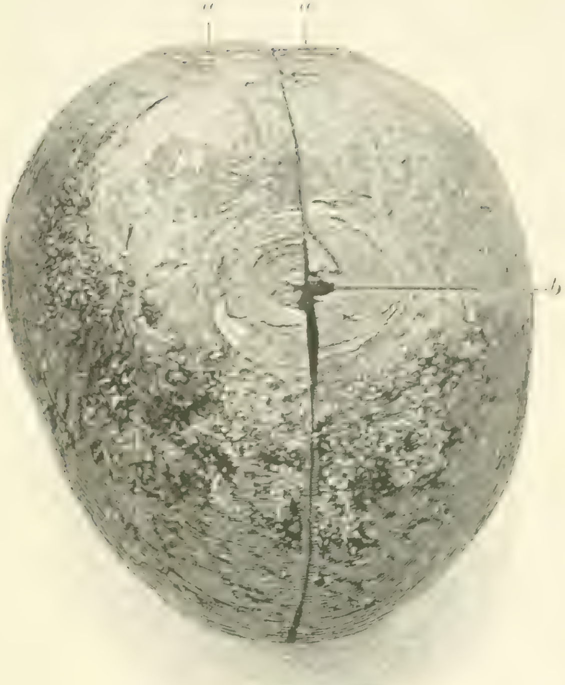

Fig. 1.—External appearance of Tumour

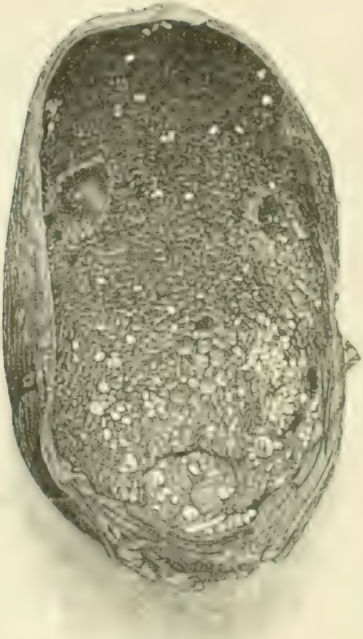

FIG. 2.—Interior of half of Capsule.

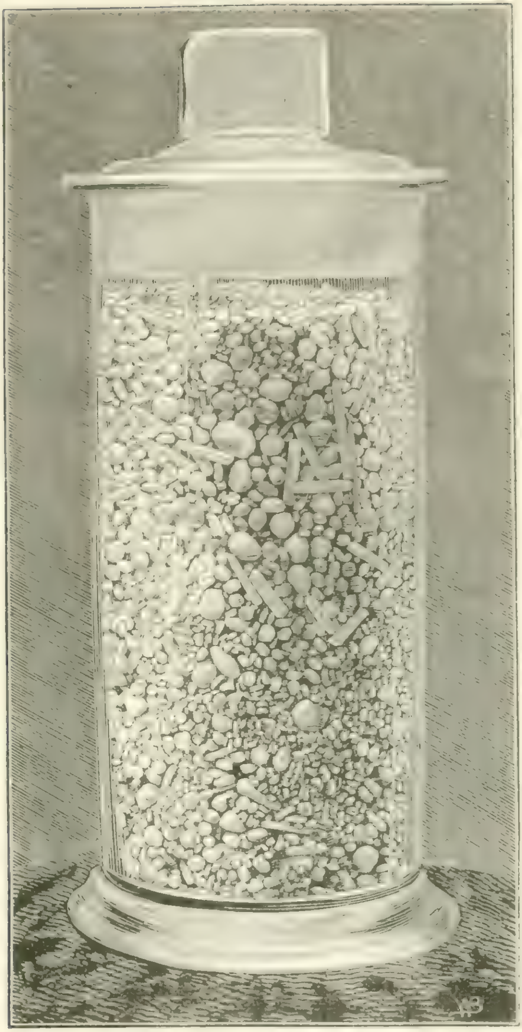

Fig. 3.—Glass Jar filled with Stones removed from the Capsule.

foreskin and the glans. The prepuce was adherent to the glans over a considerable surface.

CASE III.—In May of the present year while I was temporarily in charge of the Shantung Road Native Hospital, a male infant of about eighteen months was brought to me by its parents, who said that it had been unable to pass water for several days. On examination, I found extravasation of urine, considerable in amount, but limited to the coverings of the penis, the scrotum, and a small area of the cutaneous tissues in the groins. Sloughing had commenced in the coverings of the penis. The foreskin, which was very redundant, was much swollen and distorted, and I was unable to introduce a probe through it for any distance. I incised the upper surface, and while seeking the penis in the infiltrated and sloughing tissues, I came upon the small stone which I here figure of natural size. The urethra had given way behind the glans. After the penis was exposed, I was able to introduce a small gum catheter into the bladder, and satisfied myself that there was no other obstruction to the escape of the urine. I completed the operative interference by two free incisions in the scrotum and two less extensive ones in the groins. Extravasation not extending to the perineum, I left the tissues there untouched. For the first few days, the child's life was in considerable danger from the irritative fever that accompanied the sloughing; and it refused all food, except mother's milk,[1] which was procured for it in the native city. Eventually, it made a good recovery. For eight or ten days, the urine escaped from the point of rupture; but before the child left the hospital—in about three weeks—this was entirely closed and the urine passed naturally.

In Case I., I was much struck with the appearance of health that the patient had preserved, and with the absence of any marked sign of bladder or kidney irritation. The urine made by this man, after the operation, was clear, natural, and passed at ordinary intervals. It may fairly be doubted whether a European could have endured so long-continued obstruction to micturition, without suffering serious constitutional disturbance.

In Case II. there was a similar absence of signs of constitutional disturbance and bladder irritation; but this was, of course, much less remarkable. In spite of the adhesions formed with the glans in this case, I am inclined to regard the strictured orifice as congenital. The patient said he had had difficulty in voiding urine as far back as he could remember, and the tissues were altogether free from induration and thickening at the point.

The stones in both these cases consisted mainly of phosphates.

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[1] Human milk can usually be obtained without difficulty in China. In the native city of Shanghai, it costs at present about twenty cents for half a pint. Dr Mackenzie (Ningpo) tells me that he has frequently seen the native women milking their breasts into small basins in the streets of the native city and foreign settlement of Ningpo. It is esteemed by the Chinese as a nourishing food for old people, and for consumptives.

with only traces of uric and oxalic acids. I observe, from the reports of the Canton hospitals, that the stones removed there are usually uric-acid formations.

Cases of urethral calculus seem to be of tolerably frequent occurrence in the south. In the Canton Hospital Report for 1875, there is a case recorded in which a stone, weighing two ounces, had formed "in a sac in the urethra inside the scrotum." This was removed by enlarging "a fistulous opening at the anterior border of the scrotum." A stone of similar size had formed in the same place, and "been spontaneously discharged, by ulceration, some years previously." In Pekin, Dr Dudgeon gives an account of three cases, viz.:—In 1871, a stone, weighing fifty grains, removed by incision from the navicular fossa; the patient an adult, a native of Shantung. In 1873, a stone, weighing ten grains, removed also from the terminal portion of the urethra by incision; the patient a child of 10 years of age. In 1873, also, "A peculiar calculus, resembling a German toy-tree, with root, narrow neck, and conical apex," weighing twelve grains, extracted simply; patient an adult (Pekin Hospital Reports for 1871, page 7; and for 1873, page 10).[1] In none of the hospital reports, however, that I have had an opportunity of examining, is there a case of multiple urethral calculus recorded. My cases, Nos. I., II., and III., and at least two of those reported by Dr Dudgeon, support the observation made by Dr Kerr of Canton, that a urethral calculus is generally lodged close to the orifice. In the Canton Hospital Report for 1871, Dr Kerr writes, "Eight cases have been operated on, in which calculi were removed from the urethra. Generally, the calculus is lodged inside the meatus, but sometimes it is stopped in its course by a stricture or some other cause." Again, in the Report for 1872, he says, "Eight cases of urethral calculi have been operated on. In all of these, the calculus, formed in the bladder, had been carried to the meatus, and, being too large to pass, was detained there." On this subject, Mr Bryant, in his recently-published work on the Practice of Surgery, writes as follows:—"Extravasation of urine in childhood is almost invariably the result of an impacted calculus. The calculus is also usually arrested in the perineum. It rarely passes into the penis." Mr Bryant, however, it will be observed, limits his statements to childhood; and the explanation at once suggests itself that in children the expulsive power, voluntary and involuntary, is probably considerably less than in adults. If Mr Bryant's statement is accepted, then my case (No. III.) is exceptional, on account of the situation of the rupture. It may, I think, fairly be taken for granted that the stone in this case was the cause of the accident. I presume that it reached the situation in which I found it through the rent in the urethra, and that the urethra was opened partly by ulceration, and partly by the expulsive efforts made by the child. The stone was water-worn and grooved (see Fig., p. 681). It had probably lain fixed in the urethra for some time. Preputial calculus, to judge from surgical records, can scarcely be considered a common disease; and yet it is, of course, a condition at once established, when an individual with a contracted preputial orifice passes a stone per urethram. In the Canton Hospital Reports for 1871, five cases are recorded; and two cases are noted in the Report for 1872. In one of these last, forty-three stones were removed; in the other, fourteen.

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[1] Besides these cases, Dr Dudgeon has recorded two "median" operations, undertaken for the removal of foreign bodies (a chopstick and lead bougie) from the bladder (Pekin Hospital Reports for 1872).

*Diseases of the Eye.*—I pass now, from the consideration of a complaint that I regard as perhaps the rarest of the surgical complaints affecting the natives in Shanghai, to a brief review of a class of diseases of all others the commonest and most largely represented in the Shanghai Native Hospitals—I mean diseases of the eye. This preponderance of eye cases is not confined to Shanghai, but has apparently been observed and commented on wherever foreign surgeons have practised throughout the empire. The statement, made by Williams in 1847, "that in China, the classes of disease which most prevail are ophthalmic, cutaneous, and digestive," is, so far as my experience goes, true at the present date, at least as regards the two first divisions. The Reports of the Shanghai Chinese Hospital (elsewhere spoken of under the distinctive appellation of the Shantung Road Hospital) for the past four years are before me, and these fairly represent the work done in Shanghai. In the dispensary practice of this hospital during 1872–75, an average of 12,672 patients have been prescribed for annually, and among these the annual average of eye cases has been 1587.

Elsewhere I have spoken of "want" as concerned in the production of much of the sub-acute and chronic eye affections in China. In the Pekin Hospital Reports, Dr Dudgeon refers specially to "a too exclusive rice diet, as explaining, to some extent, the frequency of corneal ulcerations and inflammations." Other causes of eye disease have been noted by different observers, concerning which, however, I must be contented with little save a bare enumeration. Thus, in Pekin, the frequent dust-storms are said to produce much of the conjunctivitis seen there. In Pekin, also, a vicious practice of throwing quicklime into the eyes, with a view to destroy vision, has received notice. Throughout the empire generally, the transfer of the specific poison of ophthalmias is greatly favoured by the carelessness and ignorance of the natives. Judging from what I have myself seen, I suppose ophthalmia neonatorum to be of common occurrence. Syphilitic diseases of the eye are common, and I have already referred to these as probably more frequently met with here than in England. Smallpox, in spite of the progress made by vaccination, is still a scourge in China, and is the cause of many corneal ulcerations and opacities. A common practice of the native barbers, which consists in everting the lids and removing from the exposed conjunctiva any portion of mucus that adheres, is believed to be productive of much mischief. The glare of the sun, which during the summer months is peculiarly trying in Shanghai, and is also complained of at most of the open ports, is another factor that may be taken into account. A connexion between opium-smoking and amaurosis has been hinted at by one observer. Lastly, the self-mutilation of beggars has been noticed.[1] Out of this catalogue, I may, from my own experience, claim for Shanghai, mal-nutrition, specific poison of ophthalmias, syphilis, smallpox, and sun-glare.

The nature of the cases that present themselves may be gathered from the following statement of the Shanghai Chinese Hospital practice (indoor and dispensary) during the past year. In order to facilitate reference, I have taken the liberty of slightly altering the order of the classification given in the report:—

| | | |
|---|---|---|
| Catarrhal Ophthalmia | 528 ; | 22 Admitted to the Wards. |
| Purulent do. | 66 ; | 4 do. |
| Gonorrhœal do. | 6. | |
| Strumous do. | 24. | |
| Granular Ophthalmia, acute | 74. | |
| do. chronic | 346 ; | 6 Admitted to the Wards. |
| Pterygium | 48 ; | 5 do. |
| Pannus | 23. | |
| Keratitis | 252 ; | 12 Admitted to the Wards. |
| Ulcers of the Cornea | 234 ; | 10 do. |
| Opacities of the Cornea | 168. | |
| Staphyloma, total | 45. | |
| do. partial | 22. | |
| Iritis | 54 ; | 12 Admitted to the Wards. |
| Synechia and Closed Pupil | 14. | |
| Cataract | 24 ; | 6 Admitted to the Wards. |
| Amaurosis | 24. | |
| Glaucoma | 32. | |
| Lachrymal Fistula | 2 ; | 2 Admitted to the Wards. |
| do. Stricture | 12. | |
| Trichiasis | 60. | |
| Entropium | 20 ; | 20 Admitted to the Wards. |

The indifference with which the natives regard eye affections has attracted the attention of many observers; there being certainly a very marked contrast between the conduct of a European and that of a Chinaman, when sight is threatened. The Chinese males are apparently more indifferent to defects in personal appearance than

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[1] This must, I think, be of very rare occurrence. Sir Garnet Wolseley, however, in the course of a description that he gives of Singhai, the capital of Chusan, writes as follows:—"Mendicancy is a recognised institution in China. . . . One eye is frequently put out, and sometimes both, for the purpose of exciting pity." Two cases are recorded by Dr Lockhart in the Reports of the Shanghai Chinese Hospital for 1849 and 1850. Lime was the destructive agent used in these cases. I have never myself seen an example.

Europeans; but loss of beauty is of course only a part, and the least part, of the danger that threatens when an eye is diseased. Often, I think, the assurances of the foreign surgeon as to the risk of delay or neglect of treatment are not fully comprehended, or not fully trusted, by the patients. In his hospital report for 1869, Dr Johnston (Shanghai) writes—"Very frequently patients are met with in the dispensary practice suffering from severe inflammation of the eye, and on being asked to stay in hospital refuse for the most trivial reasons, although assured that without proper treatment the eyesight must inevitably be lost." I can fully endorse this statement, and so, I believe, could most of the foreign surgeons that have practised among the natives.

The disease known as pterygium is, I think, exceptionally common and peculiarly developed in China. Cases of double pterygium in one eye, single in both eyes, and double in both eyes, have all been observed in Shanghai; the two first conditions being indeed frequently met with.[1] Abscission is, so far as my experience goes, of little use in these cases, the growth almost invariably returning, and generally in a worse—more cartilaginous—form. At one time, I hoped to get good results from the tying operation suggested by Szokalski; but this also proved unsatisfactory. Latterly, I have tried "transplantation" in all my cases, and am inclined to regard this as, on the whole, the best operation. Entropium is a very common result of chronic inflammation among the natives, its occurrence being probably favoured by a peculiar disposition of the skin of the upper eyelid, to be presently noticed. The native doctors attempt its cure, but I have not myself seen any results of their interference.[2] A good many cataracts are operated on in the course of the year in Shanghai, the lens having of late been extracted chiefly by the modified linear operation of Von Graefe. In Canton, where this disease seems to be common, the hospital report for 1872 also records this as the operation usually performed; but in the report for last year—where 60 operations are chronicled—it is stated "that the section is made in the lower segment of the cornea," a statement which leads me to suppose that linear extraction has been abandoned.

Certain peculiarities in the form and position of the eye and eyelids in the Chinese have been complained of by surgeons, as constituting difficulties in operating, especially in the performance of the various operations undertaken for the removal of an opaque lens. Comparison being made with the eyes of the inhabitants of Western Europe, the peculiarities of the Chinese eye are as follows[1] :—

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[1] Dr Shearer, in the Customs Medical Report for 1871, mentions, as treated in the Kiukiang Dispensary, 56 cases of single pterygium in one eye, 16 of single pterygium in both eyes, and 11 of double pterygium in both eyes.

[2] Of these, Dr Dudgeon, in the Pekin Hospital Report for 1874, writes as follows:—"I have seen three native contrivances for obviating the condition of entropium and trichiasis: one by tying a string round the head, embracing the upper lid, and so raising it; a second by means of bamboo pliers, a fold of skin being grasped and held thus tightly, and so contracting the lid; and the third, the use of a bamboo probe, one end being fixed to the lid, and the other on the malar bone, and the bamboo forming an arch at the external side of the eye."

(1*st*.) A narrowed, often shortened, somewhat obliquely placed, palpebral aperture.

(2*d*.) A peculiar disposition of the skin of the upper eyelid, to be presently described.

(3*d*.) A wider separation between the two eyes.

So far as I know, none of these points admitting of measurement have been accurately submitted to this test; but, indeed, all that I have enumerated seem to me so apparent as to be independent of such examination, at least so far as the question of their actual existence is concerned. I have heard a diminished size of globe affirmed to exist; but this, owing to the narrowing of the opening between the lids, is doubtless more apparent than real; and although I am not at present in a position to deny it, still, without the confirmation of comparative measurements, I must regard it as a matter open to question. One surgeon has gone so far as to declare "that the lens bears an unusual proportion to the size of the eyeball;" but I think this is very doubtful. The peculiarities noticed appear to vary within rather wide limits in different parts of the Empire, and even in different individuals, natives of the same place. The most constant are the narrowing of the palpebral fissure and the wide separation of the eyes. I do not find any reference by others to peculiarities of the bony surroundings of the eye from a surgical point of view; but as two of these at least affect the operator, I note here also the comparatively slight projection of the orbital arch and slight elevation of the root of the nose. The obliquity of the Chinese eye has, of course, attracted the attention of many observers, and has been described frequently. Still, curiously enough, it appears to be a matter regarding which somewhat vague notions prevail. Not wishing to encumber my paper with quotations, I proceed here simply to note what I have myself observed.

When a well-formed English eye is opened, the small fold formed by the loose skin of the upper lid, following the movements of the rising cartilage and contracting muscle, is raised and drawn more or less deeply backwards beneath the overhanging arch of the brow, the line preserved throughout being one nearly parallel to, and at some distance from, the free edge of the lid. This fold terminates at a point above the internal canthus, its depth gradually diminishing, and extends to a point outside the external commissure (see Fig. 4). In the Chinese generally, the skin of the upper lid, owing in great measure to the slight projection of the orbital arch, descends much more evenly on a line with the skin of the forehead, and the fold which it forms, more nearly approaching the

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[1] In reading this, it must be borne in mind that my opportunities of observation have been limited to Shanghai, where, however, three provinces are well represented, viz., Quantung (Cantonese), Chikyang (Ningpo men), Kiangsu (Shanghai men).

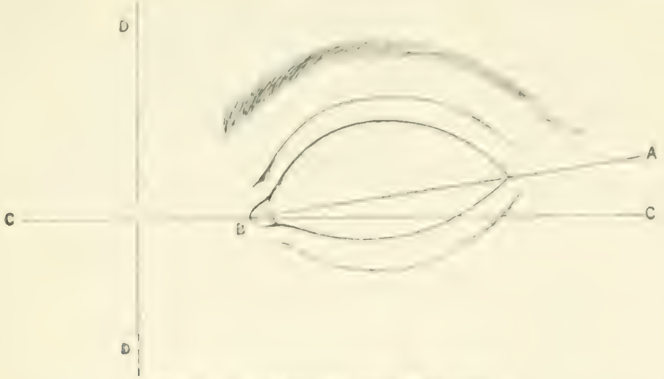

Fig. 4.—A well-formed English Eye opened.

free edge of the lid, is firmly tucked down over the internal canthus. When the eye is opened, this skin does not to any extent follow the movements of the muscle and cartilage, but remains more or less firmly stretched, the lid retiring behind the fold, which then, in well-marked examples, in part conceals the lashes. It is this peculiarity that I have supposed to constitute a predisposition to entropium. I have diagramatically represented it in Fig. 5. Hitherto, I have been unable to satisfy myself as to whether or not the free edge of the upper lid itself ever displays independently the peculiar ("sickle-shaped") curve that results from the condition just described.[1]

Apart from skin disposition, the obliquity of the palpebral aperture in the Chinese appears to be one of degree only. A straight line drawn between the centre of the internal canthus and the external commissure in the fully opened eye of an Englishman will generally be found to form an angle above a line drawn at right angles to the perpendicular axis of the face, the angle A B C, in Fig. 4; the line D D being there the perpendicular axis, and C C the line drawn at right angles to that. In the Chinese eye, this angle (A B C) is, as a rule, larger (see Fig. 5). This is, however, the utmost that I can at present say regarding it, my measurements having as yet been only roughly made. In measuring the degree of this positive obliquity, care must, of course, be taken to compare like positions of the lids. The aperture lines given in half or wholly closed eyes are in many cases horizontal, and in many fall even below the straight line (C C), such altered direction being the result of muscular movements, which depress the outer angle (commissure), and

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[1] On this point, Dr Lockhart writes decidedly—"The eyes of the Chinese are small and the eyelids drawn inwards and downwards in many cases, so as to make a distinct fold at the inner angle, as is especially seen in many of the females" (Report of Med. Miss. Society's Operations at Chusan in 1840–41).

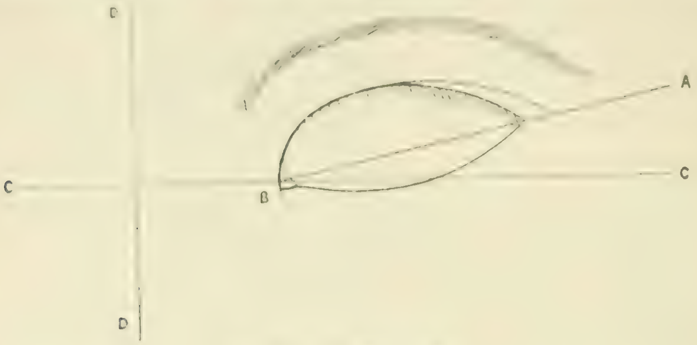

Fig. 5.—Opened Chinese Eye.

raise the inner end of the lower lid. The wrinkles of age indicate the direction of these narrowing and closing movements, especially in those that, of necessity (defective vision), or from circumstances (peculiar exposure to dust, sun, and wind), are accustomed habitually, by a voluntary effort, to diminish the opening between the lids (see Fig. 6). A certain degree of obliquity is possibly regarded by the Chinese as a beauty. Exaggerated, it is used by them to give sinister expression to masks, and the representations of malign demons. The impression that the contracted palpebral opening and the flattened skin of the upper lid give to the non-professional observer is well recorded by Sir Garnet Wolseley, in describing Prince Kung's appearance, at his celebrated meeting with Lord Elgin, in Pekin, at the close of the war of 1860:—"His features were good, being far more regular than is usual with Chinamen; but his eyes were small, and on a level with his forehead, which is the great peculiarity of his race, who may almost be said to have no eyelids, and very small apertures for their eyeballs to appear through."[1]

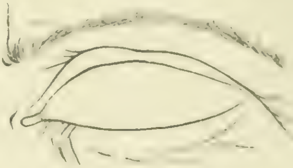

Fig. 6.—Wrinkles of Age, etc.

If, now, I have made the peculiarities of the Chinese eye clear, I have also probably succeeded in suggesting the special difficulties with which the surgeon has to contend in operating. The contracted palpebral aperture and the peculiar disposition of the skin of the upper lid occasionally cause a want of coaptation in some of the forms of eye speculums sent from home; and, in any case, imply a diminished field for manipulation, and therefore increased difficulty in operations undertaken for the removal of the lens. The slight projection of the orbital arch should, one would suppose, facilitate the use of the angular iridectomy knives, etc.; but this, I imagine, never compensates for the difficulty experienced in fully exposing the globe. During the past month, I have, on a man of sixty, performed a successful double linear extraction, on the modified plan of Von Graefe. The skin of the upper lid was relaxed, but a contracted (narrowed and shortened) palpebral aperture remained, and this was (to me) embarrassing. In excising the ball, I have constantly felt tempted to divide the commissure as a preliminary step. The division of the lachrymal canal, in "Bowman's operation," owing to the firm tucking down of the skin of the upper lid, which often, in great measure, conceals the track, becomes a more troublesome affair; and the subsequent passing of probes is now and again embarrassed from the same cause. The flattening of the root of the nose and the wide separation of the eyes doubtless alter, to some degree, the direction of the nasal duct. Let it, however, be clearly understood that I advance all these points with diffidence. My knowledge of the altogether special training required to fit a surgeon for the neat performance of ophthalmic operations prevents me from at all dogmatically insisting on the existence of difficulties that I know to be in many cases a result only of want of due dexterity on the part of the operator.

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[1] An explanation of the dependence of the peculiarities of the Chinese eye on skull formation (pyramidal) will be found in Pritchard's Natural History of Man, vol. i., page 112.

(*To be continued.*)

*Addendum*, 1*st September* 1876.—Since writing the foregoing, I am able to make an important addition to my notes on bladder-stone, having, ten days ago, performed my first lithotomy—being, I believe, also the first lithotomy done in the north of China. My patient is an ill-thriven lad, who says he is 28 years of age, but looks little more than 19. He is a native of Kongwan, a village about five miles distant from Shanghai, now to be celebrated as having been, during the past few months, connected with Shanghai by the first railway opened in China. I chose the lateral operation. The bladder was easily reached, and the stone fell at once between the blades of the forceps. Unfortunately, however, a sufficiently cautious attempt at extraction resulted in the nearly complete disintegration of the mass, which had then to be removed piece-meal, by forceps, scoop, and injections of warm water. The detritus—a soft, "mortary" mass, mixed with harder fragments, which had evidently formed an outer shell—was collected as far as possible, and weighed, when dried, an ounce and a half. The nucleus of the whole was a water-melon seed, a flat, oval body, measuring $\frac{1}{2}$ by $\frac{3}{8}$ of an inch. After all, the case was traumatic. I was reminded of a somewhat similar occurrence, of which I heard an account given some years ago. It happened in the practice of the late Mr Richard Mackenzie of Edinburgh. *Several* stones were removed by Mr Mackenzie from the bladder of an adult, and each contained, as a nucleus, a small (horse) bean. The patient stated that the beans had been introduced into his bladder by his companions while he was asleep, by way of a practical joke. My Chinese patient cannot, he says, in any way account for the presence of his melon-seed; one difficulty in his way being the fact, "that he has only eaten half a melon during the whole of last year." Judging from his past history, the seed had probably been introduced into his bladder when he was quite young. He has suffered from bladder irritation, he says, for over sixteen years; and his appearance—especially when compared with the appearance of a brother, who has come to nurse him—goes far to confirm his statements. It is now ten days since the stone was removed; and as the patient is in good health, and the wound granulating satisfactorily, I am, I presume, entitled to regard the operation as successful.

## ARTICLE III.—*On Infantile Habiliments.* By JOHN BOYD, M.D., Slamannan.

WHEN the main act in the drama of parturition has been accomplished, and another denizen of earth emerged into the circumambient atmosphere, the duty of the accoucheur is to sever the connection with the intrauterine nutrient and respiratory apparatus by cutting the umbilical cord. This done, the neonatum is consigned into the hands of the attendant matron, who proceeds to wash, dry, and dress her charge, who, in satisfactory cases, acknowledges her cares by bawling lustily. It is then usual to apply a second ligature to the attached portion of the funis, which is enclosed in a cotton or linen bandage, and pressed down on the epigastric region by a flannel binder, termed in Scotland baby's *sweeler*. Over this a small *cutty* chemise is next put on, then a loose open flannel robe called a *barry*, above which a closer garment of woollen or cotton is superimposed; the pelvic region and nether limbs are wrapped in a square of cloth folded triangularly, the utility of which is perfectly obvious. The head is covered posteriorly with a small shawl, and the infant prodigy is now ready to be admired by all and sundry.

In the days of our grandmothers another additional step was taken, which in our time has fallen into desuetude. The cranium was first covered by a small, close-fitting, cotton skullcap, entitled the *cowl*; over which was placed a more elaborate style of head-gear, adorned with drawing-strings and frills or borders, denominated the *mutch*. In some instances a small flannel pad was placed over the occiput and anterior fontanelle, sometimes between the two coverings, to make assurance doubly sure. I am informed that when the inner cap was wanted to be *wore off*, it was customary to snip a disc out with scissors day by day, so that in process of time none was left.

Differing from the elder generations, the young mothers of our own era dispense with every species of cephalic indoor habiliment, depending on the chance adaptation of the small shawl aforesaid, and on the capillary covering, scanty or luxuriant, which the new unit of humanity may bring along with it. On the reason being demanded for such a procedure, they very generally allege that the doctors forbid head-coverings, to keep the head cool. If there were any foundation in fact as to such an ordinance, the doctors would most undoubtedly lay themselves open to the accusation of thereby evincing a most unusual and unprecedented piece of sordid worldly wisdom.

It is primarily to be considered that in Scotland we inhabit a damp and coldish climate, the average temperature of which through the year, varying about 47°, constitutes a marked contrast to the intrauterine standard of 98° or 99°. The infantile power of evolving caloric being relatively extremely limited, the capacity for resisting the morbific influence of rapid reduction of temperature is correspondingly feeble. Hence it is that draughts operate so injuriously on such tiny subjects. A frosty night is seen to wither off newly-born twins like the nipping of a house-plant. Every spell of easterly wind raises the grade of infantile mortality to a most perceptible extent. Even when neither Frost nor Boreas is in the ascendant, a current from an open window, striking on the poor little bare pate, brings an attack of coryza—*snuffles*, Anglicè; *sniffters*, Scotticè.[1]

The mechanical occlusion of the nasal passages, added to the anorexia from the accompanying febrile condition, renders the baby unable to suck; its condition falls off from hour to hour. By-and-by coughing supervenes, and the wheezing, sneezing, and epiphora form a painful spectacle to the onlookers—distressing the mother to an extent sufficient to deteriorate the milk, causing vomiting and diarrhœa. To the same cause in too many cases bronchitis, lobar-pneumonia, and collapse of lung are clearly traceable.

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[1] North of the Dee, the malady referred to bears a different designation. A deceased friend, the clever and genial old minister of a parish on the Braes of the Mearns, whereof I had the medical charge many years ago, told me that in the time of my predecessor he had been at an evening party in Aberdeen, and mentioned he had been ailing; and his medical attendant, an exceedingly learned young man, had informed him that he was suffering from inflammation of the lining membrane of the frontal sinus. "Dear me!" said a lady present, "what a terrible trouble that must be! *Do* tell us, Dr H.," turning to the occupant of the chair of Physiology in the college, "what that disease is?" "It's fat we call the snō-ōkers!" quoth the Professor.

Externally, the maladies issuing from the same origin, viz. cold striking on the unprotected scalp, are, in many instances, otitis and otorrhœa, culminating sometimes in petrosal caries and meningitis.

Ophthalmia is frequently found to acknowledge no other *raison d'être*, and the marked prevalence of eczematous eruptions, and the cervical glandular tumefaction, infantile erysipelas and erythema, are streamlets of annoyance oozing from the same fountain.

On children born of the higher ranks, who inhabit well-aired nurseries, where the temperature is kept at summer heat, and well-trained nurses to regulate it, the dangers referred to may operate in an extremely minimized degree. But it is the privilege of a very small portion of our number to practise exclusively in houses of six up to mansions of sixty apartments. The bulk of the medical profession must attend to the mass of the population, nine-tenths of which north of the Tweed are dwellers in houses of from one to two apartments. Under the influence of diverging nationality, it is not altogether unexampled to find even two families crowded into one moderate-sized room. These small and crowded abodes exhibit a close and forcing heat inside, and the many crannies and ill-fitting doors and windows, while admitting a safety-valvular ventilation, cause the draughts to be so much the stronger, and the children so much the more susceptible to being injuriously affected by the rapid influx of cold external air. Now, a light but close-fitting cotton cap, adapted accurately to the superficies of the pericranium, is ever an adequate protection; no chilled perspiration nor constriction of the sub-cuticular capillaries, from too-rapidly abstracted caloric, is likely to be experienced where the snod, comfortable-looking, tidy little cap is worn.

It has been sometimes suspected that *laziness* has a great deal to do with the popularity of the present fashion of cephalic nudity, the mothers of the period being averse to the trouble of providing and arranging the caps. Some do not hesitate to ascribe it to the influence of the tone of egotism, sybaritish self-coddling, the overweening love of ease and pleasure which spread over Europe contemporaneous with, and probably originating in, the policy of the second French Empire; coinciding with the microscopic bonnets and tawdry textile profusion of female vestiary so conspicuous when Parisian fashion reigned supreme. There is no doubt but that, since the collapse of the Imperial regime, a more sensible and becoming style of female attire has prevailed, but the poor babies are still "left out in the cold."

"Finally, and to conclude," I trust I have shown cause to induce every obstetrician in Scotland to make it a point in all cases of parturition, after he has duly seen to the safety and comfort of the mother, to turn his attentions to the ministrations of the nurse, and, after the rest of the body has been suitably clothed, to tell her in a tone *stoure* enough to enforce obedience, "PUT YE ON THE CAP!"

## ARTICLE IV.—*Notes of an Epidemic of Typhoid Fever, with Special Reference to its Antipyretic Treatment.* By GEORGE HUNTER, M.D. and F.R.C.S. Edin., Linlithgow.

(*Read before the Medico-Chirurgical Society of Edinburgh, 6th December* 1876.)

THE symptoms, course, and pathological appearances in typhoid fever must be so familiar to every member present, that any reference to these would only prove tedious; but as professional opinion is still divided concerning its origin, mode of propagation, and most successful treatment, I have thought it worth while to place before the profession the following notes of an epidemic which recently occurred at Linlithgow Bridge, making special observations on these points.

Linlithgow Bridge is a village of 465 inhabitants, situated on both sides of the river Avon (which here divides the counties of Stirling and Linlithgow), rather more than a mile to the west of Linlithgow. The houses are, with few exceptions, old, and their sanitary arrangements in every respect defective. The drainage is entirely surface, by means of gutters which run along each side of the village-street towards the river. The excreta, human and bestial, are carried to midden-steads in the gardens, at the back of and in close proximity to the houses. In many instances, this ground is on a higher level than the houses themselves. Free porous gravel, overlying trap formation, forms the subsoil.

The *water* supply is from wells public and private, scattered over the village. That from which the great majority of the inhabitants on the Linlithgowshire side of the river derived their supply, was known as the "Brae Well," and had the reputation of being the best in the place. The wells known as M'Arthur's, Miller's, and Lockhart's, were made use of by those residing in their immediate proximity, especially when it became known that the Brae Well was pronounced impure. On the Stirlingshire side, the "Wester Well" was the one almost exclusively had recourse to.

The *milk* supply was obtained from six different dairies. In three of these, viz., those of Braes, Battison, and Bennie, typhoid fever early made its appearance.

For at least ten years previous to the present epidemic, there had been no serious outbreak of any zymotic disease in this village. Now and again, isolated cases came under my notice, the last of which I attended in February last, and it proved to be a mild case.

*Mode of Origin and Propagation.*—On the 17th July last, I was called to see Mrs L., a young married lady, and found that she had been ill for a week with unmistakable symptoms of typhoid. This was undoubtedly the first case of what subsequently proved a very serious epidemic. The villa in which Mrs L. lived was at a considerable distance from any of the other houses, and on this side of the river. It was comfortable and commodious, and the sanitary arrangements in every respect satisfactory. To confirm my own examination, a skilled workman was brought from town, who inspected the trappings of the drains and water-closet, and the joinings of the glazed pipes (the only ones in the village) which conveyed the sewage to the river, and no flaw could anywhere be detected. There was no accumulation outside in the shape of a middenstead, for such a depôt did not exist in Mr L.'s residence, the ashes, etc., being conveyed daily to the dungstead of an adjoining small dairy farm, occupied by Mr Braes. The milk supply was from this dairy, and all the members of the Braes family were at this time in good health. The water supply was from a private pump-well, and was found, on subsequent examination, to be free from anything like injurious contamination.

There was no other person ill of fever in the village, and the case which had occurred early in the spring was on the other side of the river, no communication having taken place in the interval between the two families.

Mrs L. was delivered of her first baby in the beginning of June last, and made a good recovery. With the exception of having been twice to Edinburgh for an hour or two's shopping, and *subsequently*, once at church, she had not been anywhere from home since her confinement. In her case, the fever ran a severe and complicated course, and she succumbed to it on the 5th August.

The next case appeared in the Braes family, whose house was on the other side of the road, about 40 yards further north than Mr L.'s from Linlithgow Bridge. There was frequent inter-communication between the two houses; on the one hand, by taking milk to the villa, on the other, by conveying ashes and certain excreta to the dungstead. Previous to being attacked, this patient had been employed about the dunghill, shovelling up and covering over what had been scratched about by the poultry.

Regarding the sanitary arrangements of this dairy farm, a few words are here necessary. The byre was at the end of and attached to the house, communication with the kitchen being obtained by a door almost directly through the gable; and through this door the odour of cows and cow manure was constantly perceptible. The dungstead was placed at the end of the line of houses, and the ground was slightly excavated for its reception. It was but seldom entirely cleared out, and a stream of liquid sewage constantly flowed into the loose gravelly soil of the adjoining field. About 40 yards in a straight line west from this dungstead, and situated at a much lower level, was the Brae well already referred to.

The next case was that of a young man named Bell, who, happening to be passing along the road on the day the lad Braes was working at the dungstead, stopped and had some conversation with him, and he became affected with typhoid on the same day that Braes was seized.

There were now three distinct *foci* of infection:—

1st, *The villa.*—Fever broke out in the house of the person who nursed Mrs L.'s infant, and spread to the neighbouring houses; and also in the coachman's family, in which no fewer than six persons were affected. (While still ill of fever, his wife was delivered of a well nourished female child, and both made good recoveries.)

2d, *The young man Bell's house*, from which several families living in the immediate vicinity were early affected, and of these one died.

It may be here interesting to note, that to this focus the first appearance of the fever on the Stirlingshire side of the river was distinctly traceable. In the house next to Bell's lived a family named Allan, and during the convalescence of Mrs Bell (who became ill after her son) Mrs Allan was seized. In the course of her illness she was visited by her mother-in-law, who resided at a considerable distance across the river. Soon after having been in her daughter-in-law's house, Mrs Allan, sen., exhibited undoubted symptoms of typhoid, and passed through a severe attack of the disease

Following closely upon old Mrs Allan's attack, fever broke out in the houses of Bennie and Battison—the only persons who kept dairies in this part of the village; and from these, and the "Wester Well" water, fever soon became general on this side of the river.

An analysis of this water was now made by an eminent chemist, and the result has here been placed before you.[1]

3d, *The house of Braes, the dairy farmer.*—In this family no fewer than six persons were struck down with the disease, and, with one exception, they were all tedious and dangerous cases, Mrs Braes succumbing to a pulmonary complication. In all these cases diarrhœa was a troublesome and marked symptom, and the evacuations, latterly after disinfection, were thrown out upon the large dungstead, which, after a fall of rain, became a pond, and soaked the soil of the field to a considerable extent. It is, therefore, more than probable that the "Brae well" got polluted from this source (as pointed out by Dr Littlejohn in his excellent report to the Local Authority), for Dr Stevenson Macadam's chemical analysis of water taken from it proved that it was contaminated with the products of the decomposition of organic matter of the nature of sewage, and that it should not be employed for cooking or drinking purposes.

[1] *Analysis of Wester Well.*

| | | |
|---|---|---|
| Saline matters, . . . . . | 15·92 | grains. |
| Organic and volatile matter, . | 2·24 | „ |
| Hardness, . . . . . | 70 | „ |
| **Nitrates decided in million parts.** | | |
| Saline ammonia, . . . . | 0·072 | „ |
| Albumenoid ammonia, . . . | 0·067 | „ |

The analysis in full I here place before you[1]

Milk from this dairy was supplied more extensively than from any in the village, and a great many families who subsequently became ill of fever traced it to this source. Churning and butter-making were carried on within a few yards of those lying ill of fever, and the same person who nursed them managed the dairy.

I may here mention, in passing, that I was able to fix with certainty the incubation period in one of these cases. A member of the Bennie family came home to Linlithgow Bridge from his situation as farm-servant in a distant part of the country, and spent Sunday with his friends. He partook of dinner and tea prepared with water from the infected well, and returned to the farm in the evening. For two weeks after, he remained in his usual health. On the beginning of the third week he had the prodromata of fever, and by the end of the same he returned to Linlithgow Bridge, had a rigor, and passed steadily through an attack of the complaint. There was no suspicion of fever anywhere in the neighbourhood of the farm at which he was a servant. The duration of the period of incubation was thus twenty-one days, which includes the time of the prodromata.

It was now considered highly desirable to have the water from the other wells submitted to reliable analysis. This was carried out, and in every instance, if we except that of Mrs L., the water was found to be contaminated with sewage or organic matter, and pronounced unfit for domestic consumption.

With such favourable conditions as filth accumulations, and impure water and milk supply, was it to be wondered at, that the fever poison spread with alarming rapidity and severity, and that out of a population of 465 inhabitants, including both sides of the river, 68 should become affected—being upwards of 14 per cent. of the whole population (I speak of my own practice only)? Of these 7 died, being a mortality of rather more than 10 per cent. of those affected.

Briefly taking a retrospect of the prognosis of the epidemic, one is confronted *in limine* with the question, how did the first case, that of Mrs L., originate? Did it begin *de novo* in her own house, or was it the result of contagion from some pre-existing case?

In the minds of the profession much difference exists regarding the mode of origin and propagation of the disease. By some physicians it is believed to be purely contagious. "It is now about

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[1] *Analysis of "Brae Well."*

| | | | |
|---|---|---|---|
| Saline matter, | 16·81 | grains | In imperial gallon. |
| Organic and volatile matters, | 2·43 | " | |
| Hardness, | 80 | " | |
| **Nitrates, decided.** | | | |
| Saline ammonia, | 0·074 | " | In million parts. |
| Albumenoid ammonia, | 0·070 | " | |

30 years," writes Dr Aitken,[1] "since M. Bretonneau related to the French Academy of Medicine a series of cases in which the communication of this disease from person to person, and its modes of propagation in this way, were so evident as to admit of no reasonable doubt." Further on he quotes Dr Watson: "If this fever be really contagious, it is not only erroneous but dangerous to hold the contrary opinion." Murchison, on the other hand, is strongly of opinion that it originates in the decomposition of organic substances; whilst Niemeyer considers that it extends by contagion as well as miasm, or that the germs which cause abdominal typhus may develop and multiply as well in the organism of a typhus patient as they would under favourable circumstances outside of it. But he thinks it doubtful if the contagion be transferred by the exhalations from the skin and lungs. Finally, Liebermeister, who gives no uncertain sound, and is much more exact in his opinions, states, "We must recognise on the one hand that typhoid fever is never contagious from person to person; and on the other hand, that it never originates spontaneously, but by a continuous transmission of the poison. The disease is not contagious in the proper sense of the word, for it is never transmitted by direct contact. It is not purely miasmatic, for external conditions alone are not sufficient to produce it. The presence of a person suffering from this disease, or of substances derived from such a person, is necessary. The poison is propagated continuously. It travels from the diseased individual to the localities which are favourable for its growth and multiplication, and from these localities again into the human body."[2]

If there be this diversity in the opinions of medical authorities, to whom can the laity look for counsel and direction to guide them in the emergency of an epidemic? Eager attempts at disinfection are made by non-professional persons, but where, how, and what to disinfect they know not; and as a result we find that chemicals with pungent and penetrating odours have been scattered broadcast where little if at all required; and where disinfection is really required, none has been had recourse to.

That typhoid fever can be transmitted directly from person to person by exhalations from the lungs or skin—*i.e.*, that it is truly contagious in the sense that smallpox and scarlet-fever are—is believed by a great section of the community, and it is to be feared by not a few members of our profession. But such a view is rendered untenable by the fact, that in several instances I found only one, or perhaps two, affected out of families comprising six or seven individuals, and where no attempt was made to prevent the most free communication between the affected and healthy. Care was always taken, however, to disinfect promptly and completely the dejections immediately after their evacuation by the patient.

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[1] Science and Practice of Medicine, vol. i. p. 410.

[2] Ziemssen's Cyclopædia, vol. i. p. 81.

Similar evidence is furnished in Fever Hospitals, where the physicians, attendants, and nurses taking charge of such cases are not more frequently affected than those who never come in contact with fever patients.

Nor does the pythogenic hypothesis of Murchison afford anything like a satisfactory explanation of all the facts and circumstances observed in the course of this epidemic. For here we have a village in the worst possible hygienic condition, with accumulations of animal decomposing substances, notably made up of excrementitious matters from byres and piggeries, as well as fæcal masses under their windows and outside their doors, a loose gravelly soil saturated with liquid sewage from such accumulations, and yet for ten years at least the public health of the inhabitants has proved itself invulnerable against any attempt at an outbreak of fever worthy the name of an epidemic. On the other hand, in the house of Mrs L. we have the most perfect sanitary conditions, free ventilation, and efficient drainage, water pronounced to be innocuous after analysis by an eminent chemist; all the conditions, indeed, which sanitary science and medical officers of health desire to obtain in order to prevent the appearance of fever, and to arrest its spread when it has commenced, and yet it is from this house that the first case takes its origin.

From a consideration, therefore, of all the circumstances and conditions which obtained in this epidemic, we are forced to the conclusion, that the fever did not originate *de novo* in Linlithgow Bridge, but was contracted by Mrs L., either on her way to or in Edinburgh (from which cases of fever are seldom absent), and that from her it became propagated continuously by the alvine evacuations. From this source every facility was afforded for the admission of the infective germs (the *ectogens*[1] in contradistinction to the *entogens* of smallpox and the like) to the water, milk, and butter supply, by percolation through the porous soil from dungsteads, by pitchers with adherent ectogens being dipped into open wells, and by direct impregnation in the dairies. If it be true, as Mr Simon affirms, that "in any specific diarrhœa, such as eminently is enteric fever, every discharge from the bowels must teem with the contagion of the disease,"[2] the practical lesson for us to inculcate upon the minds of the community is, that every discharge from the bowels of a typhoid patient should be submitted to thorough and reliable disinfection immediately on its expulsion, and that it should afterwards be buried and covered with chloride of lime in a place remote from the possibility of contaminating drinking water or its source.

*Type.*—Some idea may be gained of the type of fever that prevailed, by consideration of the number of persons affected, the more prominent symptoms exhibited by each, the duration and effect of the pyrexia, and the mortality.

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[1] Pettenkofer.

[2] Practitioner, June 1875.

The total number affected, as already stated, was 69.

Affections of the *nervous* system, in the form of slight or well-marked delirium, of the *pulmonary* organs, in the form of catarrh, hypostases, and pneumonia, and of the *abdominal* organs, as diarrhœa, hæmorrhage, and peritonitis, occurred in about the same relative proportions, or in rather more than one-half of all the cases.

The fever height never reached anything like the hyperpyrexia of acute rheumatism, the most severe cases varying only from 104·5° to 105·5°; but even this height was not well borne, and continued long in those in whom it was not modified by treatment. Regarding the *mortality*, I shall only here mention that it was slightly over 10 per cent., for it will be necessary to refer to it subsequently more in detail.

*Treatment.*—That the treatment of a disease as old as the history of medicine itself should have passed through various phases and forms, according to the beliefs entertained regarding the nature of the febrile process, is nothing more than we might have been led to expect. It would appear, however, that, dissatisfied with the doctrines and dogmas of the past, professional opinion has drifted through the troublesome times of "lowering vascular excitement" by bleeding, blistering, and sweating; it has veered round from the stupefying effects of the indiscriminate stimulation of Todd; and separating itself from the theory of casting out the entity of fever by diaphoresis, diuresis, and purgation, it has glided into the comfortable harbour of expectancy. "No medicines of any kind are required, and when not required they are often worse than useless," writes Dr George Johnson;[1] and thus relegating the principal place in the treatment of typhoid fever to careful nursing and feeding, we patiently wait for the fourteenth, or it may be the twenty-first day, in the faithful expectation that the disease shall have taken a favourable turn, and that all will now go well.

But with the application of medical thermometry to the investigation, and especially to the treatment of fevers, a new epoch has been opened up; and in none do the results promise to be more successful than in typhoid. "Sine thermometro, nulla therapia," Jurgensen lays down as the axiom, which he believes to be unassailable, in the treatment of abdominal typhus; and Liebermeister asserts[2] "that a rational treatment of this complaint without following the temperature is not possible, and that any physician who does not make two or more observations of the temperature every day neglects his duty. The common remark that such observations are applicable to hospital but not to private practice has been found to be erroneous. To measure the temperature requires so little time, that a physician who does not have the requisite leisure can hardly treat such a patient at all. A physician can really treat his patient better if he only sees him once a day, but has a good thermometric record kept by the nurse, than if he makes several visits and does not employ the thermometer."

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[1] Practitioner, 3d January 1875, On the Treatment of the Diarrhœa of Typhoid Fever.

[2] Ziemssen's Encyclopædia, vol. i. p. 81.

A knowledge of the natural history of disease has made us aware that in this, as in many acute febrile disorders, the tendency is to run a certain definite course, and to terminate spontaneously, irrespective of treatment. If, therefore, we can guide our patient through it by modifying the severity of the usual concomitants when they threaten to prove dangerous, and protect him from hurtful complications, recovery will ensue. Of these dangers, that which directly menaces life is the increase of temperature, and the burning question therefore comes to be, that of the fever.

That we may more clearly apprehend the *modus operandi* of the remedial measures employed to combat the pyrexia, it will be necessary very briefly to refer to the "Febrile process" itself.

We know that the vascular and sympathetic nervous systems are so co-related, that any influence exerted on the one speedily reacts on the other; but inasmuch as the primary motor action is vested in the latter, we designate it "vaso-motor." Putting aside the activity of the tissues, it may be stated that the regulation and distribution of the blood throughout the body is entirely dependent on these vaso-motor nerves, the centre for which is situated in the medulla oblongata, whence they pass through the ganglia to their destination. In a state of health, these two systems are so "tuned" to each other (if I may be allowed the expression), that the vaso-motor nerves only permit such a degree of blood-pressure as is compatible with a normal amount of tissue nutrition and oxidation; and a normal and unvarying temperature results. When, however, the person is submitted to the morbific influence of such a poison as that of the specific fevers, the vaso-motor nerve-centres are the first to suffer, and extreme vascular dilatation, with increased bloodflow, must necessarily ensue. Whether this results from stimulation of the centres for one set of fibres named *vaso-dilator*, or from exhaustion of those of another designated *vaso-constrictor*, it is not our business to inquire. It is sufficient for us here to know that the alteration in the tonicity of the arterial tunics so interferes with the normal conditions of endosmosis and exosmosis, that a vastly increased combustion of tissue, and therefore greatly increased production of heat, must result. But, moreover, the emunctories are so disabled, and their functions so disordered by the abnormal relations of blood and tissue, that the products of this oxidation, in a highly fermentable condition, accumulate in the circulation, and still further exercise a deleterious effect on the nerve-centres.

In thus attempting to account for the phenomena of fever by a reference to disturbances of nervous control and blood pressure, I do not ignore the theory of those who, like Drs Burdon Sanderson[1] and Binz, believe that fever "originates in living tissues, that it is from first to last a disorder of protoplasm, and that all the systemic disturbances are secondary." Of the dangers, therefore, that result from the effects of increased temperature in typhoid fever, that from consumption of the tissues of the body, due to increased combustion, may not, perhaps, be the greatest or most important. The true danger probably consists in the *rise of temperature* itself, and in the baneful influence it exerts on the protoplasm of all the organs of the body. Degeneration or even partial destruction of the cell elements of the heart, brain, liver, kidneys, and voluntary muscles, etc., have been recorded by Hoffman and Zenker; but into these the limits of this paper prevent me entering. With alterations of structure must necessarily come disturbances of function, and thus we have impending danger to heart and brain, on the continued functional integrity of which life is dependent.

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[1] Practitioner, June 1876.

From what has been said, it must be evident that no treatment can be regarded as rational or scientific which has not for its object the reduction of the abnormally increased temperature, and, as far as possible, the prevention of its production. The cold bath, and such remedies as quinine, salicylic acid, digitalis, etc., are the means to which we trust for accomplishing this end; and to the German physicians, Jürgensen and Bartels, Liebermeister, Niemeyer, and Riess, we are indebted for what we know of the antipyretic action of these agents in the treatment of typhoid.

These observers have found, that after frequently-repeated baths, commencing with a temperature of 68° F. (thermometric observations being regularly recorded), there is a decided decrease in temperature, and a marked remission of the more serious symptoms.

In an epidemic such as I have described, this mode of treatment can never become general (except, perhaps, in the case of children), both because of the want of suitable accommodation and appliances, and from the difficulty of obtaining qualified intelligent nurses to carry out the treatment prescribed. When we find that as many as twelve baths are sometimes required in one day, or more than two hundred in the course of treatment of one single case, I need not say that, in country practice at any rate, the time has not yet come for its general adoption.

It is now more than twelve months since I became aware of the antipyretic effects obtained by Liebermeister and Vogt after the employment of large doses of quinine. Until recently, I had few opportunities of putting these observations to the test, but in several cases in which I had recourse to its use during the past summer, the effects were to me sufficiently striking. In one patient—the first so treated—the disease had reached the middle of the second week when I found an evening temperature of 105·5° F., accompanied with delirium, catarrh of the smaller bronchi, and frequent cardiac action. With no little hesitation, I prescribed 45 grains of the sulphate of quinine, in divided doses of 7½ grains every ten minutes, and anxiously awaited the result. The following morning the temperature had fallen to 100·3° F.; the brain acted normally if slowly; and the pulmonary affection had almost entirely disappeared. Although the temperature subsequently rose, it never reached the same height, and the patient made a good recovery. Convinced, then, of its utility, I have employed it in a considerable number of severe cases, with the result (with very few exceptions) of reducing a temperature of 104°–105·5° F. to 100° F., and even to 98·5°; and with this decrease, a marked improvement in the general symptoms. To obtain such a result, large doses must be given at brief intervals—or it may be given in one single dose—for quinine is rapidly eliminated by the kidneys. For adults, I order, as recommended by Liebermeister, 45 grains divided into 6 powders, one of which is to be taken every five or ten minutes. Where I have considered the effects of this dose insufficient, I have ordered as much as 50 grains, without observing any bad effects beyond the physiological action of the medicine, and perhaps vomiting with the first or second dose. In addition to the ringing in the ears and increased deafness, I have observed in several cases, especially in young people, marked dilatation of the pupil, and in nearly all, general paleness of the cutaneous surface.

For every case which threatened to be at all serious in the course of this epidemic I had a chart drawn out, on which the temperature and frequency of pulse, number of stools, etc., were carefully noted at the bedside, morning and evening.

In most cases, when the temperature reached 103·5° F. in the morning, or 104·5° F. in the evening, quinine in antipyretic doses was administered. Latterly, however, finding that a temperature slightly lower, but more continuous, produced equally serious threatenings of cardiac paralysis, as evidenced by increased frequency and weakness of pulse, and hypostases of the lungs, I had recourse to the remedy even before this height was reached. Whether it be administered in the morning, as suggested by Binz, or in the evening, as recommended by Liebermeister, appears to be immaterial, for, at my next visit, a marked reduction of temperature was always sufficiently evident. More than one dose was always required; but it was seldom necessary to repeat it oftener than once in two days, and frequently not so often.

We have thus in quinine a potent and trustworthy remedy, possessing nearly all the efficacy of the cold bath, and yet almost universal in its application. In some cases, however, there is a peculiar intolerance of it, even in much smaller doses than those mentioned; and this, together with its high price, makes it desirable to have at our disposal a medicine which is equally safe and trustworthy as an antipyretic, and yet procurable at a more moderate cost. In salicylic acid, I believe, we have such an agency.

Without having been aware of the published observations of

Riess of Berlin on the action of salicylate of soda in the treatment of this disease, I was led to make trial of the acid from my experience of its beneficial action in rheumatic fever.

It may be employed in the form of (1) *salicylic acid*, which is an insoluble yellowish white powder, without odour, and of a sweetish taste. It is readily soluble in water containing an alkali in solution, and thus, with soda, we obtain (2) *salicylate of soda*, a neutral salt. (3) As *salicylate of quinia* I have also used it, but in this form it is expensive, and not more antipyretic than either of the preceding.

It may also be prescribed in the form of ammonia or potass salts; but the lowering action of the potass on a heart already feeble seemed to contraindicate its use in the only case in which I had recourse to it. My experience leads me to prefer the soluble soda salt, since we now know that, in the presence of the carbonic acid of the blood, salicylic acid is set free.

Like quinine, it must be given in full and frequently-repeated doses. For an adult, 10 to 20 grains are given every ten minutes until 6 doses are taken. For a child of 5 years I have given as much as a drachm and a half in an hour without any toxic effects resulting; but in two instances—both children—slight croupiness of inspiration followed, probably the result of catarrh of the pharynx and upper part of the larynx. Abundant diaphoresis has attended its employment in two or three of my patients. Tried at intervals of one or two hours, no antipyretic action was noteworthy, proving that it is rapidly eliminated. In nearly all the cases treated with salicylic acid, the pulse fell with the temperature, delirium of a mild character gave way. Symptoms of pulmonary œdema and hypostatic congestion became less anxious complications, and in these respects I found a decided resemblance to the action of quinine. Salicylic acid is, moreover, a valuable astringent, and is therefore peculiarly indicated in those cases where the diarrhœa is frequent and exhausting. After its use the dejections become more consistent, and usually do not exceed one or two in the 24 hours. This of itself is a strong point in its favour, since the chances of the formation of bedsores are lessened, the duties of the attendants are lightened, and the risks of propagating the disease are *a priori* diminished.

I append charts of four cases where the remedy was employed,[1] and on these its power of reducing temperature and pulse will be seen at a glance.

Bearing in mind what I have said regarding the "process of fever," the mode of action of quinine in causing decrease of temperature will now be intelligible. Its power of causing contraction of unstriped muscular fibre is manifest in its action on the uterus and pupil; and we are entitled to conclude that it has a similar action on the arterioles. In this way we explain the paleness of cutaneous surface, which equally results from antipyretic doses of the drug, and from the use of the cold bath. With diminished blood-flow there must be lessened combustion, and this, with its power of arresting or destroying the migratory movements of the white corpuscles (which according to Binz are the seat of energetic oxidation), may account for the anti-oxidizing influence of quinine. But, apart altogether from its action on the vessels, quinine in common with salicylic acid possesses a powerful antizymotic and antiseptic effect on the tissues themselves.

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[1] Kindly prepared for me by my assistant, Mr J. C. P. Muir.

The recent investigations of Klein[1] have resulted in the discovery that a vigorous absorption of peculiar organisms, which are really *fungi*, takes place in the intestine over and around Peyer's patches, and that these pass into the lymphatics and veins of the mucous membrane. It is thus not improbable that salicylic acid and quinine, when given in substance, may also act locally in destroying these organisms, and that the intestinal secretions may be submitted to an antiseptic action before being expelled *per anum*.

Before leaving this part of the subject, I cannot too strongly impress on those desirous of applying this treatment to their typhoid cases, the necessity of having early recourse to these remedies, *i.e.*, before a rise in the pulse rate to 120 or upwards indicates the commencement of cardiac paralysis, and its train of dangers. As Dr Clifford Allbutt truly expresses it,[2] "to allow a patient to continue in a high early temperature, is often to consent to his death."

Regarding the effect on the *mortality* after the employment of antipyretics, time only permits of my stating, that of the 7 fatal cases

2 died of peritonitis—one of these with intestinal hæmorrhage.
1 ,, of intestinal hæmorrhage and collapse.
2 ,, of pulmonary œdema and hypostatic congestion.
1 ,, of cardiac paralysis, and
1 ,, of cerebral, pulmonary, and abdominal complications.

Thus, out of 68 persons affected, the disease was fatal in 10 per cent. Comparing this with the results obtained by Liebermeister in the hospital at Basle, we find that,

| | | | | |
|---|---|---|---|---|
| Under expectant treatment, the mortality was | 27 per cent. |
| ,, incomplete antipyretic, | ,, | ,, | 16 | ,, |
| ,, sytematic antipyretic, | ,, | ,, | 8 | ,, |

While thus placing prominently before you my limited experience of the practice of these German physicians, let it not be supposed that a rational treatment of typhoid consists merely in the daily machine-like recording of temperature, or that the labours and lesson of Louis and Trousseau, of Jenner, Murchison, and Begbie, are ignored or undervalued, because they are unmentioned.

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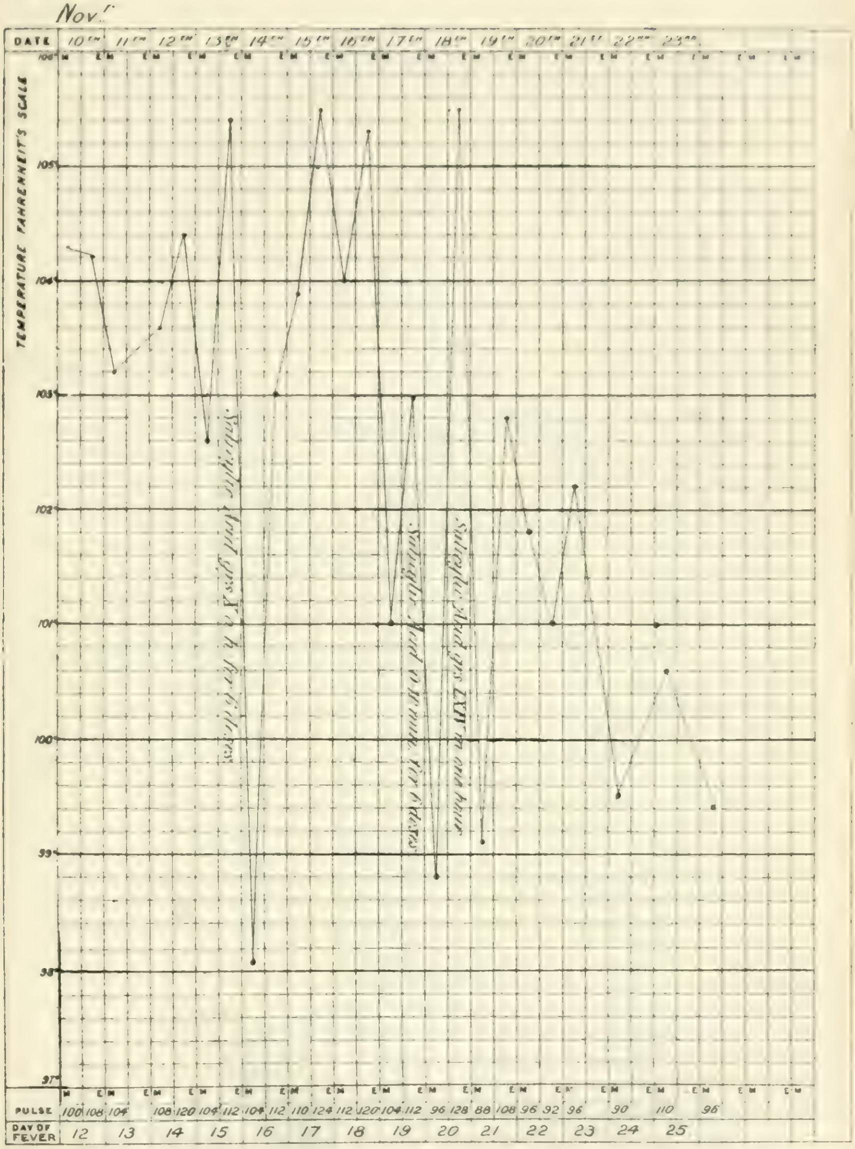
[1] Practitioner, January 1876.

[2] *Ibid.*, January 1871.

A.B.

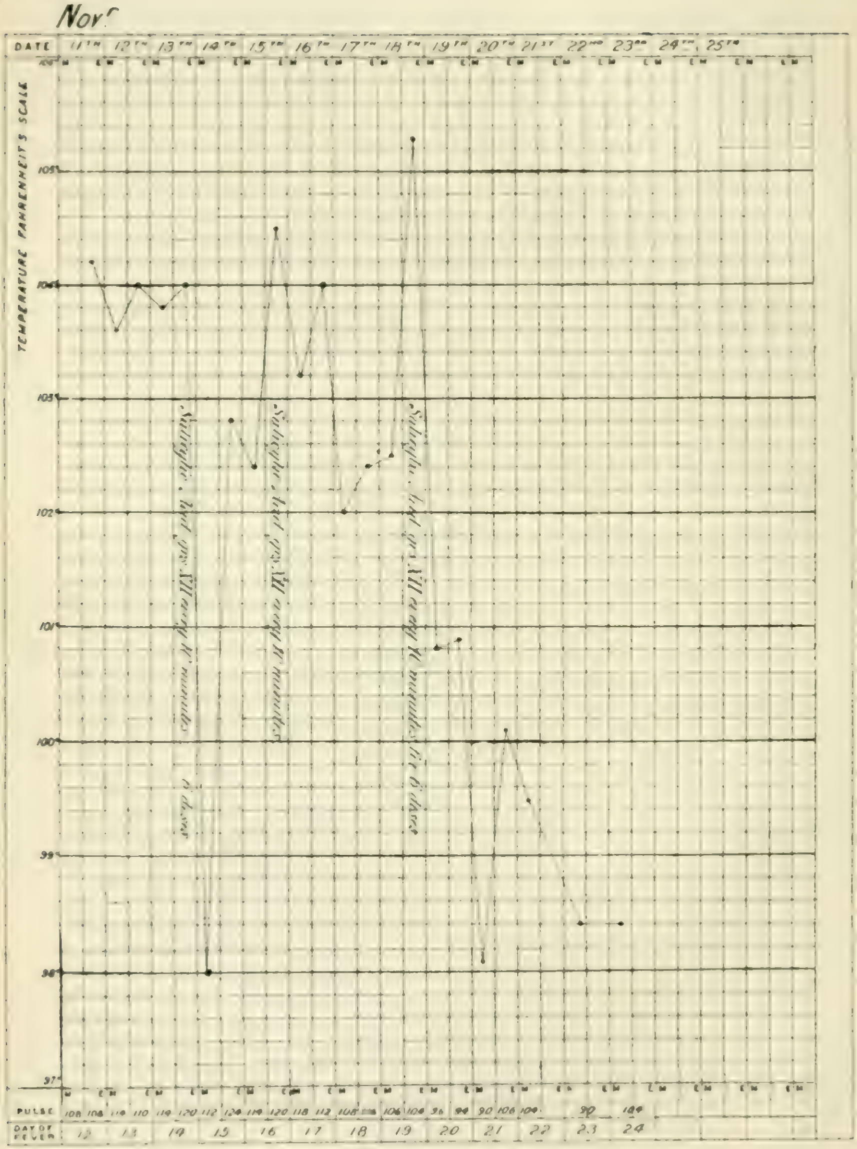


Salicylic Acid grs X e h for 6 doses

Salicylic Acid 10 grms. for 6 doses

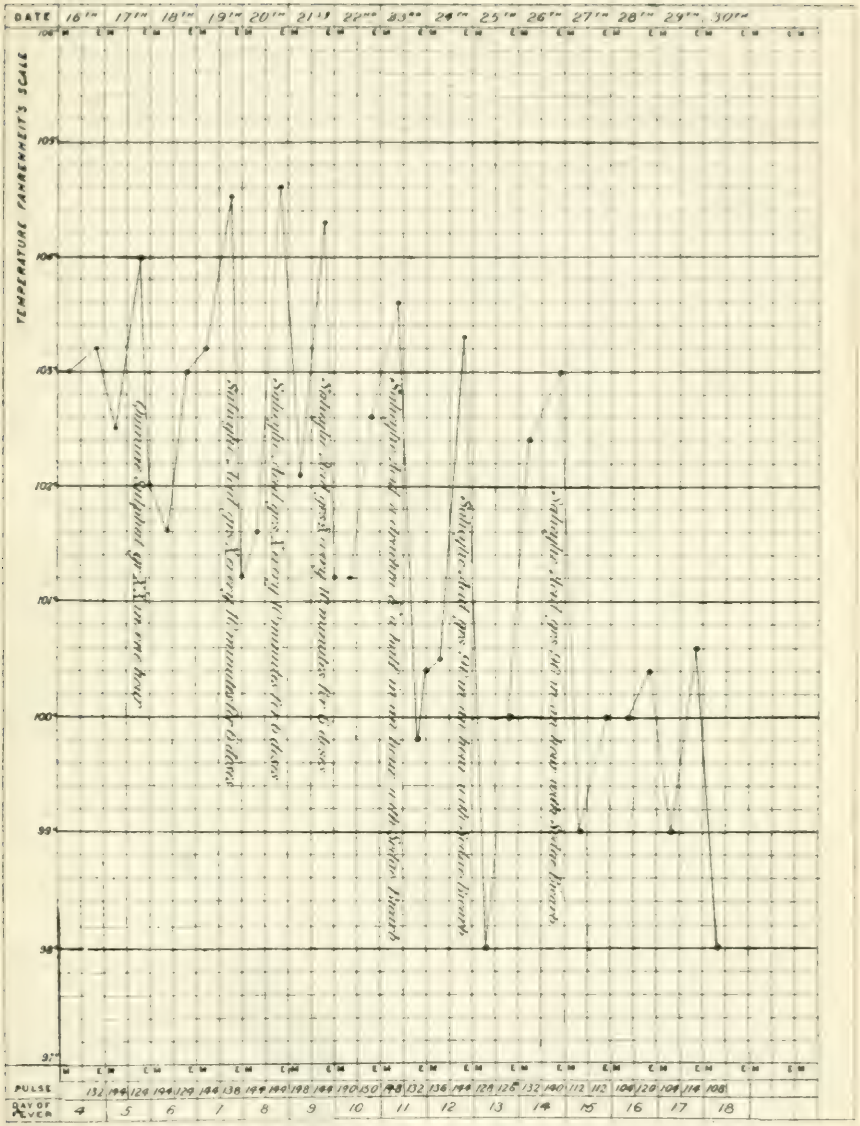
Salicylic Acid grs LXIV in one hour

I. B


Novr
DATE
11th 12th 13th 14th 15th 16th 17th 18th 19th 20th 21st 22nd 23rd 24th 25th
TEMPERATURE FAHRENHEIT'S SCALE
105°
104°
103°
102°
101°
100°
99°
98°
97°
Salicylic acid grs. XII every 4 minutes 6 doses
Salicylic acid grs. XII every 4 minutes
Salicylic acid grs. XII every 4 minutes for 6 doses
PULSE
108 108 114 110 114 120 112 124 114 120 118 112 108 106 104 96 94 90 106 104 90 104
DAY OF FEVER
12 13 14 15 16 17 18 19 20 21 22 23 24

B.S

Novr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TEMPERATURE FAHRENHEIT'S SCALE
106°
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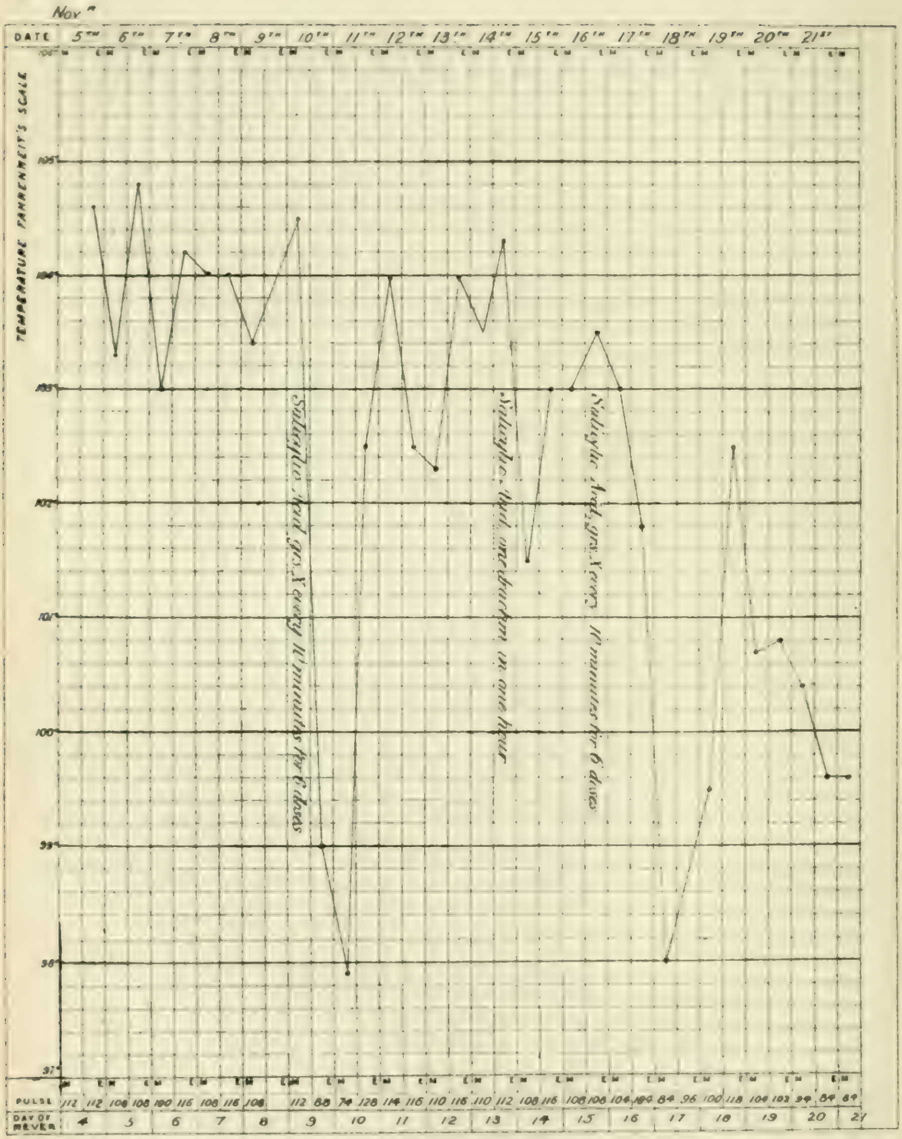
103°
102°
101°
100°
99°
98°
97°
Quinine Sulphat gr XX in one hour
Salicylic Acid grs X every 10 minutes for 6 doses
Salicylic Acid grs X every 10 minutes for 6 doses
Salicylic Acid grs X every 10 minutes for 6 doses
Salicylic Acid a drachm in a half an hour with Sodae Bicarb
Salicylic Acid grs 60 in an hour with Sodae Bicarb
Salicylic Acid grs 60 in an hour with Sodae Bicarb.
PULSE
132 144 124 144 129 144 138 144 144 198 144 190 150 148 132 136 144 128 126 132 140 112 112 104 120 104 114 108
DAY OF FEVER
4 5 6 7 8 9 10 11 12 13 14 15 16 17 18

J.M.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E M
TEMPERATURE FAHRENHEIT'S SCALE
106°
105°
104°
103°
102°
101°
100°
99°
98°
97°
Salicylic Acid grs. X every 10 minutes for 6 doses
Salicylic Acid one drachm in one hour
Salicylic Acid grs. X every 10 minutes for 6 doses
PULSE
112 112 108 108 100 116 108 116 108 112 88 74 128 114 116 110 116 110 112 108 116 108 108 104 104 84 96 100 118 104 102 94 84 84
DAY OF FEVER
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If we would successfully do battle with this too frequently fatal (and yet preventible) disease, we must avail ourselves of all the juvantia of our art; and to no single physician are we as a profession more indebted than to the great Dublin master, who, while he taught us how to tranquillize delirium and relieve tympanitis, yet, with that modesty so characteristic of a noble nature and an exalted mind, desired no better epitaph than that "HE FED FEVER."

## ARTICLE V.—*On the Distinction between Inflammatory and Diphtheritic Croup.* By ROBERT TURNER, M.D., Keith.

"NOTHING," said Canning, "is so treacherous as *a fact*, except, perhaps, *a figure*." In view of the existing complex croupo-diphtherial entanglement among medical writers, and, what is worse, among medical practitioners, which Sir John Rose Cormack so lucidly exposes in his recent communications to this Journal, may we not parody the great statesman's witty aphorism and say, as regards nosology, "Nothing is so treacherous as *a name*, except, perhaps, *a symptom*"? I do not, however, consider this *quæstio vexata* a hopelessly involved one, now that it has been laid fully before us; the "wound" is "probed," and that is a first step towards "bringing the plaster." The force of individual opinion, *based on observation*, I believe, if it could be widely enough evoked, might do the rest. Under this impression, I beg to offer my own views on the point at issue, stating briefly the grounds on which they have been formed, as a well-intended, if slight, contribution to the pursuit of an end so desirable.

In my student days, when arming for battle with disease, I heard much of *the false membrane of croup*, held to be observable on the throat, or expectorated in the form of casts of the trachea, more or less perfect, in the course of an attack, or found in these situations after death. I do not remember that any of my pastors and masters said they had *seen* it, but they were all at one in inculcating the belief in its constant presence in severe cases, as the product of simple inflammatory action; and the teaching of all the best-known systematic writers of the time was to the same effect. On engaging in practice, I found the sthenic inflammatory croup (of children) to be a common disease in these parts—ushered in by febrile excitement, and often hoarseness for a day or so—yielding, in many cases, to an emetic and warm bath, the first continued, perhaps, in nauseating doses; and when resisting these measures, best treated by topical bleeding, blistering, and calomel and Dover's powder. While ministering to such cases I sought diligently for the redoubted *false membrane* as long as the quest seemed of any promise, but *I never found it*, either on the throat or in the sputa. Its invariable absence was puzzling, and I lived in a state of bewilderment about it for more than a dozen years, at a loss whether to consider said "membrane" a myth, or a reality which had eluded my search. A day of enlightenment came, when I had ceased to strive for it, in the manner following:—

Before 1846, diphtheria was wholly unknown *practically* in this district,—nay, I think I may affirm, in all the north of Scotland, —to the present race of practitioners at least; and we had no intimation of its having appeared in any part of the kingdom from the Medical Journals or other sources of information. Early in the spring of that year, cases of seemingly the usual croup occurred in my practice with unwonted frequency, chiefly over a low-lying, swampy part of the country, and, what was very unusual indeed, in two and sometimes more members of a family at the same time. In a few early instances I dealt with the invader as I had been accustomed to deal with inflammatory croup, never doubting the accuracy of my diagnosis; for, as it happened, these cases all occurred in children: the laryngeal symptoms set in early, and no serious constitutional disturbance was manifest—a concurrence of circumstances, I would fain believe, fitted to mislead one in my position. Suspicion, however, being soon aroused, I examined a throat on the first opportunity, when the *diphtherite* of Bretonneau, hitherto known to me only by *reputation*, became a personal acquaintance; and from that day to the present our intercourse has not flagged for any great length of time. For the first few months, my cases, perhaps naturally as marking the *rise* of the epidemic, were of the sort I have described; afterwards, the disease showed itself in all its severity, local and constitutional, and in adults as well as in children. It has continued to do so at irregular intervals ever since.

Meanwhile the old, non-contagious, non-membranous croup of our juvenile population holds on the "even tenor of its way," uninfluenced in any manner or degree by the companionship of the new-comer, being still as prevalent in this quarter; unchanged in its etiology, semeiology, and measure of fatality (which is small when compared with that of diphtheria), as when I first encountered it more than forty years ago.

The conclusion at which I have arrived on the important question which forms the subject of this paper may be gleaned from the foregoing history and commentary. I need not, therefore, occupy space in stating them formally; but I may add regarding them, if only in support of my claim to be heard on the subject of diagnosis in diphtheria in this its day of hot discussion, that they are not of late or of hasty growth; for so long ago as 1854, I pointed out (in the Association Medical Journal for 28th July of that year) the distinctive features of inflammatory and diphtheritic croup, and all I have since seen of both has but served to confirm me in the opinions then expressed.

## ARTICLE VI.—*On the External Use of Salicylate of Iron.* By ROBERT KIRK, M.B., House Surgeon, Royal Infirmary.

SALICYLIC ACID and its compounds have been used for some considerable time as antipyretics and antiseptics, and as such have fulfilled very satisfactorily what was demanded of them; but still they had the disadvantage, shared also by carbolic acid, of being in no way astringent, and therefore of allowing free capillary bleeding after operation, unless pressure was employed. An astringent antiseptic seemed therefore to be desirable, and after experiments with a number of salts, trial was made of the salicylate of iron, which seems to have both actions in an almost equal degree.

By adding salicylate of soda to a saturated solution of sulphate of iron a double decomposition takes place, and sulphate of soda and salicylate of iron are obtained in solution, from which the latter may be easily separated by crystallization. This is, however, quite unnecessary, as the solution just mentioned is a most convenient form for application, and one which can be prepared in a few moments. Of a bright claret colour, with no smell, and with hardly any irritant properties even when used in strong solutions, it is not so repulsive as some of the more generally used disinfectants and antiseptics. Its antiseptic properties are most easily demonstrated, for urine containing but a small quantity of this salt in solution will remain long free from bacteria when exposed to the same influences as those which are conducive to the life of bacteria generally.

The cases to which this salt has been applied have generally been open sores, often with more or less unhealthy action going on, but in each the progress towards recovery has been very rapid after the first few days. As an illustrative case I may be allowed to give the following:—

Sarah W., age 67, was admitted into Ward 1, Royal Infirmary, under the care of Mr Joseph Bell, on 31st October, suffering from two varicose ulcers on the right leg. The larger ulcer, about the size of half-a-crown, was situated midway between the ankle and knee on the inner side of the limb; and the smaller, the size of a florin, was about two inches lower. The surrounding parts were inflamed, while the ulcers themselves were devoid of granulations, and painful. Rest was at once enjoined, and black wash was employed until the end of November, when the salicylate of iron was employed instead. The ulcers were then as large as florins, and still were almost free from granulations. For the first three days after the solution was employed, there was but little evident progress made as far as the size of the sores was concerned; but the surfaces became more healthy, granulations formed, and the edges became softer. Cicatrization now followed rapidly, a decided difference being noticed every day, and the patient was dismissed on 13th December, the sores being completely healed, although the skin surrounding them was covered with a red crust, which, however, was rapidly removed with tepid water. The dressing used in this case was that of lint soaked in the solution, and covered with guttapercha tissue. Were I disposed, I could easily multiply cases of cures as rapid as that quoted, but I do not think this at all necessary, as the case, I think, speaks for itself.

In conclusion, I must state that I owe my opportunities for the application of this salt to the courtesy of Mr Joseph Bell, who kindly permitted me to use it in many of the cases in his wards.

## ARTICLE VII.—*Note on the Treatment of Rupture of the Ligamentum Patellæ.* By JOHN CHIENE, Assistant-Surgeon, Edinburgh Royal Infirmary.

J. F., aged forty-four, was admitted to the Surgical Clinical Wards on the 8th of September 1876. He limped into hospital, complaining that something had given way in his right knee. He stated that, shortly before his admission, in going down a ladder his right foot caught on the last step, and that he fell forwards, his right leg bending under him. On examining the joint, the nature of the accident was at once evident. The patella lay on the anterior surface of the femur above the condyles; there was a distinct gap between the patella and the tubercle of the tibia. The ligamentum patellæ could not be felt. The patient could not extend his leg.

*Treatment.*—An oblong piece of strong extension plaster, large enough to cover the anterior and lateral aspects of the thigh, was heated, and fixed in position by means of a roller bandage. It was shaped so as to embrace the patella, and to its distal corners, on either side of the knee-joint, two pieces of strong indiarubber tubing 8 inches in length were attached by tapes. The limb was then laid on a posterior splint with footpiece, and slung in the inclined position to a wire cradle. The indiarubber tubing was

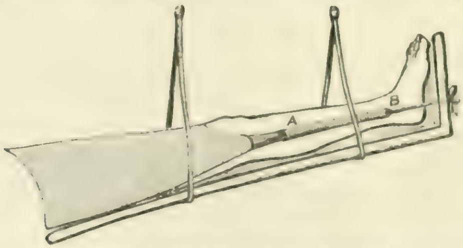

Diagram of Limb laid on the posterior splint in inclined position, illustrating the shape of the sticking plaster attached to the thigh; the position of the indiarubber tubing (marked A B); and the mode of fixing the tubing to the foot-piece. The roller bandage fixing limb to splint is omitted.

fastened by means of tapes to the footpiece, and tightened sufficiently to bring the patella to its normal position. The elasticity of the indiarubber relieved the feeling of rigidity, and its contractility counteracted any loosening of the apparatus due to stretching of the tapes or slipping of the plaster.

The patient was in bed eight weeks. The plaster required renewal once. He never had any uneasiness from the apparatus, and was discharged on the 4th of November with firm union. Measurement from the upper border of the patella to the tubercle of the tibia was 4 inches on both limbs. On the 3d of January 1877 the patient was shown at the Medico-Chirurgical Society, with complete restoration of flexion and extension at the knee-joint. The measurement from the upper border of the patella to the tubercle of the tibia was now 4½ inches, slight stretching of the newly-formed material having taken place since 4th November.

I have also used this simple method in a case of fracture of the patella with an equally satisfactory result, simply drawing the upper fragment down to the lower, and applying nothing to fix or push up the lower fragment.

## ARTICLE VIII.—*Original Observations and Experiments on the Nervous System, and on the Process of Innervation and Nutrition.* By ERNEST HOUSTON FORJETT, late Resident Physician, Fever House, Royal Infirmary, Edinburgh.

### PAPER I.

IN this paper the experiments are briefly and simply related. They are divided into three series.

I. Take an animal, cut down upon and deposit in the floor of the fourth ventricle any colouring matter devoid of irritating or other injurious properties. Make the opening into the ventricle as small as possible, and close it effectually by artificial means, so that, after the external parts are united, there will be no draining away of the cerebro-spinal fluid. The operation is an extremely delicate one. Out of fifty attempts on different animals, only *five* gave satisfactory results. If the animal lives through the first forty-eight hours, and seems at the end of that time in tolerable health, it will in a few days—within ten is all we can say—be ready for microscopic examination. At the post-mortem, we found, in all the cases which proved unsuccessful, the colouring matter either just as it had been deposited, or lying among the muscles and fascia of the nape of the neck. In those which proved successful, the colouring matter was not, or only little of it, seen in the site of the operation; and on submitting the animal to microscopic investigation, the following results were observed: *In the spinal cord*—Transverse sections made of it throughout its entire length showed in some of the cuttings the colouring matter deposited here and there, and in other cases it occupied the entire field, both in the axis or band and in the white substance. The same state of matters was observed in sections from the larger nerve-trunks. In an ordinary section of these parts, granular masses of various form, and cell-like bodies of many sizes, are seen filling the whole space. We say cell-like, for in reality they are not cells; they possess neither cell walls nor fluid contents; they are solid, spherical collections, and masses of atoms drawn together by the law of mutual "molecular attraction." The colouring matter in the cases above mentioned assumed granular and cell-like forms, presenting unipolar, bipolar, and multipolar prolongations, standing out in bold relief, and presenting a striking contrast side-by-side with the other colourless structures of a similar type. But the chief results were found in the peripheral terminations or ultimate tubules of the nerves. These seemed to consist of a row of cell-like bodies, or of small granular forms of annular consistence, held in position or restricted by the neurilemma, and placed at definite distances apart; the smaller the nerve the closer the bodies were. These bodies were enveloped with the colouring matter, the intervening spaces almost clear. Well, then, in the spinal canal and larger nerve tubes there were found protoplasmic bodies with a greater or less number of arms, and of various shapes and forms, while in the terminal nerves only round bodies. To these two facts we beg particular attention. *In the lungs*—The parenchyma of the lung is composed of lobules of various sizes, which, although closely connected together by an interlobular areolar tissue of fimbriated substance, are quite distinct from one another, and can in the fœtus be easily separated. On viewing an external section of the lung with a power of 30 diam., we found that the spaces round, or rather the borders of the smaller lobules, which, in the normal condition, are filled with a serous fluid, were tinted with the colouring matter. A transverse section showed the spaces between the ventricles likewise tinted. And on a third section of the same lung we saw dark spots scattered throughout the field, which, no doubt, were particles of the colouring material. *In the liver*—This organ is composed of lobules made up of minute cells, which are held together by extremely fine areolar tissue or fimbriated substance. Between the interlobular spaces, the minute passages of Chrzonszczewsky, there exists a serous fluid, which we found tinted with the colouring matter. *In muscle*—This is composed of fibrillæ, which are made up by the regular apposition, side by side, of the dark and light particles of their ultimate structure. The fibrillæ, coalescing, form a muscular fibre or a fasciculus. Each of these is surrounded by a structureless membrane, called by Bowman sarcolemma. According to Kuhne, the sarcolemma contains a semi-fluid, the muscle structure, which he terms muscle-plasma. This semi-fluid we have found tinged with our colouring matter. *In the glands*—We have not yet succeeded in making a satisfactory section of these organs, though we have met with a certain amount of success in the salivary bodies. In three cases we found dark spots on our section which undoubtedly could have been caused only by the colouring matter; and this point is furthered by the fact that, on two occasions we found the mouth, tongue, and gums stained. *The kidneys.*—Neither have we succeeded with these organs, yet might add that we have found the urine frequently stained with the colouring matter.

II. The second series of operations are closely connected to the above. Let the operation be performed after the manner already described, but the aperture is to be left open. In every operation on the brain which is followed by loss of the cerebro-spinal fluid, the first symptom to be observed is that the animal's breathing becomes difficult and laboured, stertorous. The same is to be observed during the influence of chloroform. In the first series of operations where the aperture is closed, the animal will, if it be strong and healthy, in a few hours show signs of revival, which will be greatly accelerated by the inhalation of the nitrate of amyl, and with a little attention will rapidly recover. In the present series, the train of incidents is as follows:—First, as already stated, the breathing is altered; then the animal, on awakening from the effects of the chloroform, will seem to be in a satisfactory way towards recovery; perchance it will even eat if food is offered; but in the space of an hour or so the ordinary symptoms of cerebral anaemia supervene. That it is suffering from cerebral anaemia, and that owing merely to loss of the cerebro-spinal fluid, is evident from the fact, 1st, that the animal was in perfect health prior to the operation, and only some time after the draining away of the fluid did the symptoms in point present themselves; 2d, that it was not the effect of the chloroform is obvious, as it had recovered from it; 3d, it is proved by the nitrate of amyl relieving the symptoms. Well, the ordinary symptoms and signs of cerebral anaemia are witnessed. The animal sits huddled together, with its eyes closed, and panting. If you approach it, it will open its eyes and shut them almost immediately. If you disturb it, it will endeavour to run away; but ere it has gone a few yards, it will be seized with a true cataleptic fit. This state of matters can also be induced by lifting the animal by the ears. The nitrate of amyl at once relieves this condition. If the animal is left in perfect rest, it will remain in this semi-comatose state for twelve to twenty hours; at the end of this time it begins to show signs of restlessness; the muscles twitch violently, and if a spoonful of milk be applied to its mouth, it will lap it, but the process of swallowing is performed with difficulty. Violent nervous tetanic excitement soon follows, as portrayed by the thumping and kicking-out of its feet, and the head droops and sways from side to side; then the right side gives way, hemiplegia commencing with the right fore-leg, as is denoted by the limb being stretched out; the right hind-leg is next involved, and if it is not supported by artificial means, will topple over on to this side. If allowed to roll over, it makes violent efforts to regain its natural position; and though at first it may succeed, it does so only to fall again with the full force of a dead weight. Its strength soon gives, and it remains panting in this position, the right limbs powerless, but the left ones are convulsed; next, paraplegia rapidly sets in, then follows general paralysis, succeeded by death by asphyxia (primarily, coma: secondarily, apnœa)—which generally results in not less than forty-eight hours after the operation. If stimulants in any form be administerred, death is brought about more rapidly. The longer death is delayed, the better will the parts be for microscopic examination. From first to last, the animal has full possession of its instincts. On examination, the blood will be found thinned and impaired, the muscles and fascia to have assumed either a state of incipient fatty degeneration and metamorphoses, or broken down into the form of molecular debris, and, in addition, the round protoplasmic bodies of the blood will be observed much altered both in structure and in property.

III. Kill an animal with electricity. The longer the animal has been kept under the influence of the current prior to demise, so will the granular and molecular particles be of larger form, their contour more unequal, and their branches more numerous and spindle, and the cell-like bodies be of larger size. In the brain, spinal cord, and larger nerves, these are easily detected, while in the periphery not much change is to be observed. The same alterations of structure in form and degree are to be noted in subjects who prior to death have suffered from long-continued neuralgic and rheumatic pains, as also in animals run to death in the chase. The opposite in cases of sudden death, as when struck by lightning.

In Paper II. and III. we will treat of the Process of Innervation and Nutrition as demonstrated by the above experiments.

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## ARTICLE IX.—*Three Cases of Pneumothorax occurring as an early Symptom of Pulmonary Disease.* By ANGUS MACDONALD, M.D., F.R.C.P.E., F.R.S.E., Physician to the Royal Hospital for Sick Children, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 3*d January* 1877.)

PNEUMOTHORAX is so frequently met with as a symptom of advanced phthisis, whether tubercular or otherwise, that we are apt to regard its occurrence in pure medicine as all but necessarily associated with such conditions. We forget that every now and then the affection may be an initial symptom, no doubt usually of evil prognostic import, but still not exactly one of the elements of that complexus of terrible intensity that forms the close of a case of subacute or chronic phthisis. The following three cases are the whole of this kind that have come under my notice in the course of eleven years' private practice; and, being aware that they are rare, I have judged that it might not be uninteresting to the members of this Society to hear the record of them.

Every physician who attempts to study cases closely in private practice, and more especially to record his observations and experience, does so at an immense disadvantage, and with many drawbacks. Still the thing is possible to a wonderful degree, and I certainly do not care to subscribe unhesitatingly to the pessimist view, lately put forward by a most distinguished teacher in this city. Energy and assiduity are a match for any obstacles that can arise; and private practice affords opportunity for observation and research in many important classes of disease that never find their way to hospital, or at least do not get there except in such a degree of aggravated intensity that they are useful merely as a pathological and not as a therapeutical study. I must say, that I sometimes feel the air of conscious superiority with which hospital physicians and surgeons speak of what they call *practitioners*, as anything but agreeable or warranted. The possession of the advantages of an hospital practice are inestimable alike to patients to physicians and surgeons; but wherever disease presents itself it can and ought to be studied, and the results of treatment can often—I am sorry to say not in those cases I am going to lay before you—be gauged with a juster standard than they can be in an hospital.

But though I am putting on this bold front, I am not sure but it is dictated by a good deal of the same spirit which makes the schoolboy whistle to keep his courage up, or which, to be more classical, induced the "Fairshon" to select such a disproportionate number of musicians to warriors in that now famous attack of his upon the herds of the Sassenach.

At any rate, I must crave your indulgence for the imperfect notice which is given of my second case, the truth being that the case was written down about the time of the young man's death, from memory, as I had neglected to take notes of it. The other two were written out at the time.

With these preliminary remarks, I proceed to record my cases in chronological sequence.

### CASE I.—*Sudden Pneumothorax, Consecutive Pleurisy, Consolidation at Right Apex. Recovery.*

Mr J. S., M.A., aged 35, student of divinity, Edinburgh, enjoyed fairly good health till about middle of March 1868, when he was seized with a severe pain in the chest and back on getting up in the morning. He remained in the house for a few days, applying of his own accord hartshorn and oil with apparently some advantage. When first seen in consequence of this illness, he complained of severe pain over a spot on the right side near the nipple. There was no friction audible, and it looked to me like a pleurodynia.

for which it was treated by mild external counter-irritation.[1] On the day following this examination I was called to see my patient, who then resided in 60 Cumberland Street. I found the pain had become worse, and was accompanied with extreme breathlessness. The right side of the chest was hyper-resonant throughout, and considerably distended. There was amphoric breathing and occasional metallic tinkling—in fact, indisputable evidence of pneumothorax. The chest, so far as examination then made could ascertain, was sound, and I had had occasion, about a fortnight previously, to examine it with great care. I now got Dr Warburton Begbie to see him. A bandage was ordered, and quiet to be maintained. According to a record furnished me by the patient himself afterwards, it would appear that he kept the house for six weeks, with his chest bandaged tightly, and had no pain, only an uneasy feeling, which he likened to the lungs sticking to the ribs. About this time he saw Dr Begbie a second time, who declared he was much better; and after another fortnight he went to Aberdeen, where his health improved rapidly, and he was able to walk about nearly the whole day. He returned to Edinburgh about the middle of May, when the air was found to be quite gone. About a fortnight after this he felt a sharp pain close by the spine on the right side, which gradually increased, and extended over the whole lung. On examination, loud friction was audible, and other evidence of an extensive dry pleurisy. Iodine ointment was applied over the surface of the right side of the chest. The patient began to get thinner and weaker. About 20th July he went to Banffshire, and for a fortnight was hardly able to move about—partly, however, owing to the heat, which was unusually great. His rest at night was much disturbed. About the middle of August he was seen by Dr Watt Black, who told him there was fluid within the pleura, at its lower part, and that the apex of the right lung was considerably consolidated. Blistering ointment was at first applied, and afterwards a fly-blister to the lower part of the right lung, and at the same time croton-oil over the apex. Dr Watt Black saw him a month after this, and found less fluid and consolidation; and, seeing him again three weeks later, Dr Black told him that he considered him progressing favourably towards recovery. He returned to Edinburgh about 28th of October, much improved in strength and with a good appetite, but still feeling pain in his right side, though he was convinced it was diminishing every day. After his return he continued to improve. I examined him on the 23d of December 1869, with the following results:—

Flattening under the right clavicle, with comparative dulness and increased vocal resonance for two inches downwards. There was also dulness below a transverse line passing a half-inch below the right nipple. Expiration somewhat prolonged, and breathing somewhat bronchial at apex. At base breath sounds were rather weak; vocal resonance somewhat diminished up to the middle-third behind. There was no friction sound anywhere in the chest nor pectoriloquy. No crepitation at apex. The general health is remarkably good. No night-sweats.

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[1] Mr S. merely called at my house on the way to his college class.

*23d February* 1870.—Breathing under the right clavicle decidedly clearer than on last examination. No crepitation. No night-sweats. General health has continued good. Dulness under right clavicle seems less marked.

*26th May* 1870.—Dulness under right clavicle simply comparative. Expiration prolonged, but very slight increase of vocal resonance, and there is no crepitation. Breathing otherwise normal. General health good. Has gained in flesh and strength, but does not find himself fit for severe application to study.

*27th June* 1873.—Patient seen by Dr Begbie along with me, who, on being shown the note I had taken of his case in May 1870, pronounced him *in statu quo tunc.*

The patient left for New Zealand in the early autumn of 1873 to take charge of a congregation in that colony.

I had a letter from my patient, dated New Zealand, 25th October 1875, in which he tells me, among other things, that he is successfully working a parish 30 miles long by fifteen broad; that he preaches three times on Sabbath; and rides for three days each week on an average 20 miles a day, in the prosecution of his vocation, and finds his health excellent.

That certainly does not indicate any increase of chest weakness. So much for the result in this patient's case.

### CASE II.—*Pleurisy, Pneumothorax, Pyo-pneumothorax, and Death.*

Mr A. F., æt. 23, consulted me in the spring of 1871 on account of his health. I found evidence of weakness at the left apex, and sent him to the country for the summer. He belonged to Ross-shire, and passed the summer there with great advantage to his health. He returned in the autumn in very good condition, resumed his duties as a clerk in one of the large life assurance offices of this city, and, after examination by the medical officer, was admitted to the benefit of the institution by means of a small assurance on his life. But his cough soon returned in such great severity as to attract the attention of his fellow-clerks; and on 2d December 1871 Mr F. came to my consulting-room complaining of a severe pain in the left side. I found friction audible over the painful area, and ordered him not to return to his office, but go home; and I, at the same time, prescribed the usual treatment for pleurisy.

On calling at his residence next day, I found him much worse. Since I had seen him the day before, he was seized with a severe fit of coughing, and along with this came a sudden increase of pain and difficulty of breathing. On examining his chest I found plain evidence of intense pneumothorax. This at first was dry, but subsequently was accompanied by a large amount of fluid.

The cough, phthisical symptoms, and dyspnœa increased; we had both metallic tinkling and Hippocratic succussion present. With the assistance of Dr Affleck I tapped him on two separate occasions to relieve the urgent dyspnœa. The first tapping was serous, the second sero-purulent. He never improved on the whole, however, and lingered on till the 10th of June 1872, when he died of exhaustion.

### CASE III.—*Pleurisy, Pneumothorax, Death.*

A. B., æt. 25, consulted me in November 1871 on account of a slight hæmoptysis. I found a certain amount of dulness at the right apex. The hæmoptysis was treated by an internal styptic, and it never returned.

It was within my knowledge that one of his sisters died of phthisis, and I had been consulted by other two of them with considerable hæmoptysis. On these grounds, as well as because I was not able to satisfy myself that the original lesion at the right apex had entirely disappeared, I rejected a proposal of assurance on his life made in 1874 to an assurance company for which I act. His health, however, continued good, and there was no recurrence of the hæmoptysis, and without reference to me Mr B. was, I believe, admitted to the benefits of a life policy in an Edinburgh office shortly after this rejection.

In March 1876, after a report from me in regard to his family history, and a statement that I believed there was little, if indeed anything, of the original disturbance at the right apex remaining, he was admitted to a policy in another Edinburgh office, with 5s. per cent. per annum of a loading; which proceeding has been proved to have been perfectly justified, so far as the lungs were concerned.

In June 1876 Mr B. consulted me on account of pain on the right side, in the region of the right nipple. I found evidence of a slight dry pleurisy. The pain went away in a day or two, but was followed early in July by an extensive dry pleuritic seizure, which compelled me to keep my patient in bed for some time. This attack was accompanied with some cough, but no other pulmonary symptoms, and with very little fever.

After recovery he left for Perthshire on the 1st of August 1876. On the 3d of August he was seized with a severe fit of coughing, followed by instantaneous and severe pain in the right side, along with breathlessness. This attack was looked upon by the local medical man as due to bronchitis and disease of the liver.

He did not improve, and, becoming restless, he returned to Edinburgh on the 8th. He was then seen by two medical friends, as I happened to be in the country, and a certain degree of pneumonia of the right lung was diagnosed, but it was at the same time noticed that the right side was hyper-resonant in front, and the breathing peculiar. The pneumothorax, however, escaped detection.

His breathing was extremely imperfect and rapid. His temperature 102-103°; pulse 130-140. In short, the patient appeared to be dying. He was also possessed of a curiously absurd idea, that he had been poisoned by medicines administered in the country. Supporting treatment was adopted, and he rather began to improve.

I returned to town late on the 14th August, and visited Mr B. early on the 15th. I found him extremely weak. Pulse 130; temperature 103°. Respiration very hurried, but I neglected to count them. Tongue furred, much vomiting, and a little diarrhœa. Appetite extremely bad.

*Examination of chest gave following results*:—Breathing on left side puerile, but otherwise nothing abnormal.

Right side of chest distended and fixed. *Anteriorly and laterally*, the intercostal spaces projecting. Vocal thrill and resonance diminished, percussion note tympanitic, especially towards the lower third, over the area where hepatic dulness ought to be. Breathing weak, but distinctly amphoric, over the whole of the right side anteriorly.

*Posteriorly.*—Percussion dull below the level of the upper edge of the scapula, above that line hyper-resonant. Moist sounds audible over area of dulness, and vocal resonance increased. The test of the clang elicited by striking one coin against another was very clearly marked, when the ear was applied over the supra-spinous fossa, and the coins knocked against one another on any part of the anterior surface of the right lung. But if the ear of the observer was applied anywhere below this level on the posterior aspect of the right lung, the test entirely failed. It is to be noted that there was nowhere any metallic tinkle, and that the very weak state of the patient rendered it advisable to dispense with all unnecessary examination, especially to make the examination of the back as brief as possible.

I ordered a continuation of the supporting treatment. Ice for the vomiting. White of egg beat up in milk; lime water and milk; beeftea; chlorodyne in repeated small doses for the cough; and a tight bandage to be placed round the lower part of the ribs.

He continued for some time in this precarious condition, but gradually improved, and on the 14th of September, the following was the state of his health:—

Improving very decidedly; pulse 72, regular, fair in strength. Temperature 98·4°. Tongue clean and moist. Coughs slightly in the morning, but does not sweat at night. Eats and sleeps well. Is taking quinine, and feels getting stronger.

*Chest.*—Left side breathing puerile, but no abnormal condition traceable.

Right side *anteriorly*.—Percussion resonance greatly increased along the anterior aspect of the chest, as far down as to within an inch of the lower edge of the false ribs. Lower edge of the liver on a level with the umbilicus. Vocal thrill and resonance distinctly diminished. Respiratory sounds somewhat feeble, but breathing distinctly amphoric; loudly so at the lower part of the hyper-resonant area, faintly so at the upper part. At the lower portion the voice-sounds evinced the peculiar amphoric after-clang, observed in such cases. Along the side of the chest no amphoric breathing audible, though the breath-sounds are weak, and the vocal resonance and thrill diminished, and the percussion note is preternaturally clear.

*Posteriorly.*—In supraspinous fossa percussion clear, but perhaps not abnormally so. Below this space the percussion note is flatter than on left side. Vocal thrill and resonance relatively to the left side diminished *even* over the *dull* area. The breathing is harsh, and partly bronchial towards the middle and lower part of the region of dulness. It is, if anything, weaker than normal in the supraspinous area, and is there occasionally slightly amphoric. But in this space the vocal resonance is not diminished. The coin test is still applicable, but in a much less degree of perfection. No metallic tinkle. Hippocratic succussion tried. No splash heard.

*23d September.*—Left side as before. Right side *anteriorly.*—Percussion hyper-resonant to level of the nipple, and some slight amphoric character in the breathing, which is weak. Below nipple percussion note defective in resonance. No amphoric breathing, breath-sounds weak, no increase of vocal resonance. *Posteriorly.*—Percussion hyper-resonant in the supraspinous fossa; below that defection in resonance. Coarse crepitation audible over the middle-third of the scapular region. Breath-sounds at base weak. Pulse 120, weak. Cough very troublesome. Tongue clean.

*8th October.*—Patient a good deal better, and has been able to get out. Crepitation on right side posteriorly, pain and dulness are less. The amount of air, judging from the physical signs in the right pleural cavity, is much less, and seems to be chiefly restricted to the lower third of the cavity.

*16th October.*—Patient has got worse since last examination. Pain complained of on pressing upon abdomen, and there is much distension. The latter came on rather suddenly on the evening of the 13th, and at first seemed merely due to flatulence, but tenderness on pressure has been increasing for the last day or two. He has latterly been falling off in flesh, and has become very restless. Cough very troublesome. Expectoration profuse, frothy, and only occasionally yellowish. Tongue dryish, clean, and red. Experiences an uneasy feeling after taking food, and the cough is always worse then. Bowels alternately loose and bound.

Unmistakable evidence of return of the pneumothorax in all its original intensity. No metallic tinkling; a curious sound, half like friction, half like crepitation, audible, accompanying respiration over the middle and base of right lung posteriorly.

No true crepitation audible anywhere.

Was ordered a mixture containing chloral, bromide of potassium, and squills, to allay the nervous restlessness and to subdue the cough. Nutrients diligently continued, and poultice over abdomen. Heart very disturbed. No valvular murmur. Organ is pushed somewhat to left.

*19th October* 1876.—Since the 16th till last night, patient had been easier; pulse 120, temperature 103°. He is to-day much worse. Pulse 144; extremely feeble. Lips and face blue. Respirations 48 per minute. Can lie only on right side. Left lung shows slight dulness at base posteriorly, and all over its surface the breathing is extremely harsh, with prolonged expiratory murmurs, and slight crepitation at the end of expiration. The cough is most incessant, with a tendency to faint. The right chest seems much the same as at last examination, only the amphoric breathing is now heard most loudly over the lower half of the lung posteriorly. There is no metallic tinkle. Abdominal tenderness gone. Patient slept none last night. Ordered 5 gr. chloral hydrate, and 15 gr. potassium bromide every six hours, milk and chicken-tea as frequently as possible, as also a dessertspoonful of spirits in water when the faintish feeling came on. Poultices to be applied to left side. Some nourishment every one and a half hour during the night.

*20th October.*—In the morning, left lung found in a state of complete congestion. Patient sank at 2 P.M.

A post-mortem examination, forty-seven hours after death, was performed by Dr Wyllie, with the following results. Chest only was allowed to be examined:—

Body slightly emaciated. On reflecting the soft parts from the chest, and carefully cutting through the intercostals, there was a sudden escape of gas immediately when the right pleural sac was opened. The right lung was completely collapsed, being reduced to about one-fifth of its natural bulk, and pressed in the form of a flattened cake against the right side of the spinal column. Along its posterior surface it was bound to the chest by adhesions; elsewhere on its anterior and lateral surface it was covered with a thick layer of soft and recent lymph. Beneath this layer the visceral pleura was thick and opaque, as if from inflammation of older standing. On the lateral surface of the lung, about midway between apex and base, and at the posterior part of the lateral surface, there was a round yellow spot about the size of a halfpenny. This was raised in a disc-like manner above the general surface, and in its centre there was a ragged opening about an inch in length, from which a quantity of grumous pus exuded, when the lung was pressed upon. This opening communicated with a cavity beneath the yellow spot, and extended into the lung substance to a considerable depth. The cavity was about the size of a small apple, and it had soft, rather ragged walls. On its outer surface, immediately beneath the yellow spot, there was a greyish spot about the size of a chestnut. The cavity communicated at its inner extremity with a bronchial tube of medium size. The whole remaining substance of the lung was in a condition of complete atelectasis, being rather dry and leathery in texture; but there was nowhere any tubercle to be found. About a pint of an opaque grumous fluid was contained in the pleural sac. Left lung was voluminous, its substance extremely congested and œdematous. Pericardium contained about three or four ounces of serum. Heart about normal size, but right side much dilated; all the chambers filled with dark blood and soft coagula. Aortic valves competent. No appearance of thrombosis in any of the chambers. Mitral and tricuspid valves natural. The structure of the raised disc, examined by Dr Wyllie, gave the anatomical characters of a slough.

I have thus laid before you the leading details of the above three cases, believing as I have said that such cases are remarkably rare, and that it is the duty of every physician to contribute, however few they may be, any observations of general interest that fall under his eye.

The symptoms in all three cases were so clear as luckily to exclude all doubt in regard to diagnosis, so that we are completely warranted in asserting in the most categorical manner, that here we had three cases of extensive pneumothorax, occurring, in one case, as a complication of tubercular phthisis before it had reached the second stage; and in the other two cases, certainly in the one as proved by post-mortem examination, and presumably in the other, from its very successful issue, altogether independently of tubercle.

In proof of the early occurrence of this disease in the second case, I may be allowed to draw attention to the fact, that though the chest showed some slightly doubtful symptoms in early spring of 1871, yet residence in the country so improved my patient that he believed himself perfectly well, and at any rate looked so. He was free from cough, fat and strong, and so little suspicion was there of any radical mischief being present, that he was passed for a life policy by one of our best Edinburgh physicians. With the return of the cold weather of autumn, he had a relapse in the symptoms of cough and pulmonary irritation. On the 2d of December 1871, I had the opportunity to witness the evidence of already forming pleurisy, which we are entitled to regard, with Dr Gairdner[1] and others, as nature's method of preventing such an accident as occurred in the course of the following night. The pleuritic exudation had, however, not proved a sufficient barrier to the air under the repeated high degrees of tension to which it was exposed by the severe efforts of coughing, and so the lung had given way at the point where we may assume that a tubercular nodule was in process of softening. I ought to notice that the course of the case indicates here tubercular and not pneumonic phthisis. There was nothing made out indicative of antecedent cold; and, moreover, though the cough was very annoying previously to the formation of the pneumo-thorax, the expectoration was only slight; thus showing that it arose more from the irritation of existing tubercle than from the accumulation of mucus or of cheesy deposit.

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[1] Gairdner's Clinical Medicine, p. 397.

Though, however, such a case is interesting and possessed of some value, I regard the other two as of far more importance, and to really form considerable additions to our rather scanty records of such cases.

My first case may almost be placed alongside that of Dr Thorburn of Manchester, which Professor Gairdner,[1] in his remarks upon it, agrees with Dr Thorburn in describing as a unique case. It is also of great value, as it was very carefully watched by me for a series of years. Besides this, Dr Warburton Begbie saw the patient on repeated occasions, and confirmed my diagnosis. Also Professor Arthur Gamgee, on one occasion, was taken by me to see the patient; so that all doubts of diagnosis must be dispelled from even the most sceptical critic. Furthermore, it seems to me that this case illustrates in the most precise manner possible the natural history of the cure of pneumothorax, as indicated by Professor Gairdner, in his paper on that subject, already quoted, as the probable course of events. It will be noticed, on reference to the record, that we had first a local attack of pain near the right nipple. In this case, unlike that of No. II., there was not a friction murmur audible, but only symptoms of pleurodynia. There was curiously, however, no cough and no wasting, nor other evidence of pulmonary disease. Indeed, I happened to have carefully examined this gentleman's chest about a fortnight previously—for what reason I forget—but I recollected that my examination led me to the conclusion that his chest was perfectly sound. From some cause or other a perforation then took place, preceded by a feeble effort at the formation of pleurisy; for so I interpret what I at the time believed to be a pleurodynia. This pneumothorax gave rise to considerable disturbance at first in the shape of pain, breathlessness, and distension of the right chest, the air being apparently absorbed; but the patient's condition gradually got better, to be followed, however, by a continuation of the pleuritic mischief, which here seems to have been more of the nature of cure than of disease. This pleurisy, accompanied with a certain amount of effusion, traversed inwards, and gave rise to consecutive pneumonic condensation, which, as it occurred at the right apex, was at first feared to be due to tubercular deposit. The subsequent history, however, almost negatives such an opinion. We have, therefore, in succession in this case, the following occurrences:—Rupture of the lung and visceral pleura, from some cause difficult to determine, but to my mind most probably trivial, preceded by limited pleurisy at the seat of rupture, and followed by extensive pleurisy; healing of the lung perforation as a result of the pleurisy; absorption of the imprisoned air by some means; absorption of the fluid secreted into the pleural sac in consequence of the pleurisy; re-distension of the lung to nearly its natural condition, with only a slight degree of permanent pneumonic condensation at the right apex, left as an evidence of the strange pathological chain along which the right lung had been made to pass. So much for case No. I.

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[1] Loc. cit., p. 385.

Case No. III. is also extremely interesting, and more particularly so because we were fortunate enough to obtain a sectio, and thus were able to gauge with complete accuracy the state of matters. I may here state that, in this case, looking at the family history, and having regard to the general views in respect to this lesion, as well as the condition of health of my patient, I was prepared to find the presence of tubercle in the lung. But to my surprise there was none. It becomes, therefore, a question of very great interest, and very difficult of solution, How was this pulmonary abscess and sloughing of the pleura occasioned in Mr B.'s case? Two answers, after careful consideration, present themselves with a certain degree of probability. The one is, that the starting-point had been due to embolic infarction of the part; the other is, that, from some cause or other, there had occurred at the spot such an intense amount of haemorrhage into the lung-tissue as led to death of the part, and subsequent formation of abscess and perforation of the pleura. In the endeavour to elicit support for the first view, we examined with the greatest care into the history of the case, with the expectation of finding some clue to its causation in the existence of a scratch or abscess in any part of the patient's body, which would have warranted us in assuming the probability of any septic origin for such an embolism. But we could find nothing of the kind. Then there was no cardiac lesion to warrant the assumption on the score either of escape of vegetations from any valve, or of the formation of auricular emboli, owing to undue slowness of cardiac action. We were, therefore, I think, driven from the embolic theory of the perforating abscess by want of any evidence to support its probability.

The history of the patient's family, and also his own, rather incline to support the haemorrhagic theory. It is within my experience now, that three of his sisters required to be treated, some once, some oftener, for hæmoptysis. He himself first consulted me for a hæmoptysis, which at that time came from the right apex. But on inquiry at his friends, I find that, though he never informed me of the fact, he was all along liable to hæmoptysis now and again. He had a morbid dread of consumption, and believing hæmoptysis to be a sign of it, he endeavoured to conceal the occurrence of it, and deluded himself into the conviction that the blood which he expectorated came from his throat. Having therefore made out such a predisposition to hæmorrhage in Mr B., it seems to me, in the face of the difficulty of laying one's hands upon a more likely cause, that it is more probable that the lung lesion was originated by hæmorrhage than by anything else. I may here add, that his nervous system was very peculiar, and rather weak. Of course I merely throw out this as a probable cause, and do so for want of being able to think of a better explanation.

But be this true or the reverse, it is clear that we had here a local inflammatory lung lesion lit up, non-tubercular in its nature, which, as it approached the surface, set up at first a slight, and subsequently a large, amount of pleurisy, and ended by perforation of the pleura, notwithstanding it had been so strengthened to resist the rupture. The antecedent pleurisy and consequent adhesions, which, however, before the death had got broken up by the pneumothorax, modified in a very peculiar manner the distribution of the air within the pleura for a considerable time, and consequently made the diagnosis a little difficult. To explain the condition of matters on his return from the country, it seemed to me at the time I saw him first, that, besides being bound down to the wall of the chest posteriorly, the lung was then also adherent, with a considerable degree of firmness along the lateral, and possibly, also, along a considerable part of its anterior surface; and seeing that the sectio revealed the lung "along its posterior surface bound to the chest by adhesions," and "elsewhere on its anterior and lateral surface covered with a thick layer of recent lymph," and "beneath this layer the visceral pleura thick and opaque, as if from inflammation of older standing," my original opinion is thereby strengthened. In consequence of this adhesion, the air was confined to the anterior surface of the lung, and even there was not able to accumulate, especially towards the apex, in any very great amount. A very slight amount only of air was thus able to get round the right apex, and accumulate in the supraspinous fossa. The lung, after the perforation, was further evidently for a time thrown into the condition of catarrhal pneumonia, or at least of congestive catarrh. With this condition of his lung, it being fixed to the chest posteriorly, laterally, and partly anteriorly, and there being a large perforation in its pleural surface, and a considerable amount of air in the pleura, the patient returned from the country at the end of the first week of August. The condensed, semi-collapsed right lung, being thus bound down posteriorly, did not communicate the peculiarly delicate coin-test clink, except the ear was held at the extreme apex posteriorly, where a little free air reached, and there it became distinctly audible wherever along the anterior surface of the lung the one coin was made to impinge upon the other. The amphoric breathing, also, was weak towards the upper part of the lung, owing, as I believe, to the circumstance that as yet the adhesions had not allowed more than a thin layer of air to get into the pleura at the upper part of the chest, while it was always loudly present at the lower part, where the adhesions seem to have never existed to any great amount.

The fact of dulness, with relatively increased vocal resonance, being observed at first over the right back, and subsequently the same dulness, with relatively diminished vocal resonance, is explained, I consider, by the other evidence (from fine crepitation, etc.) of the existence of a degree of pneumonia in the former case, and of simple pulmonary collapse in the latter. Adding a still further peculiarity, must be put the fact, that at no time in this patient's case could I hear metallic tinkling, though I sought for it with extreme care; and though it was well pronounced in the other two cases I have reported. I may also state here that though I carefully tried the succussion test, I was unable to elicit it. I did not do so the day before death, as the patient was too ill, but I did so on 15th September, as I have noted, and I almost think on some other occasions. No doubt but this test would have been applicable the day before death, but the higher claims of humanity forbade its being tried. This test also was very markedly present in my second case, after the pleurisy went on to effusion.

A word or two still in regard to the absence of metallic tinkling. Throughout the whole history of this case, there was no difficulty in making out amphoric resonance, as well as the echo of the voice—the *nachklang* of German authors—but true metallic tinkling I never was able to hear, though every time I examined the patient's chest I watched for it. That occasionally pneumothorax occurred without metallic tinkling, was known to Laennec; and he explains that he has found it absent, or nearly so, under two sets of conditions, its place being then taken by amphoric resonance, or, as he calls it, Bourdonnement amphoric.[1]

1. When there are two or more fistulous communications between the cavity occupied by the air and the bronchi. 2. When this cavity is very large, and contains only a small quantity of liquid.

I need hardly remark, that Laennec's notion that the presence of fluid along with air in the pleura, or in a pulmonic cavity, is necessary for the production of metallic tinkling, is denied by Skoda, and does not seem to be well founded.[2] His belief also that for its production it was absolutely necessary that the air cavity should communicate directly with the external air, either through a bronchus or through the wall of the chest, is denied by the same great German physician. Skoda believes that the perforation in the lung in pneumothorax seldom remains patent, and that the air vibrating in a bronchus may communicate its condition to the air in the pleural cavity even when it is separated entirely from the bronchus by means of a very thin layer of tissue. These views he supports by reference to an easy experiment on the stomach distended with air, when metallic tinkling and amphoric resonance may be heard in the stomach when a person speaks into a stethoscope placed upon the organ; and he says the tinkling and echo are heard all the same, whether the stomach contains a certain amount of fluid along with the air, or contains air only. Be this as it may, it is seldom in pneumothorax that we have not both the amphoric resonance and the metallic tinkling. The latter is frequently more or less intermittent in its occurrence, the former is usually constant. But in the case before us we found constant absence of the metallic tinkle, and that, too, even the day before death, when, judging from the post-mortem appearances, it is scarcely possible to believe that there was not a certain amount of fluid along with the air in the pleural sac.

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[1] Laennec's words are—"Les cas où la résonnance existe seule ou beaucoup plus habituellement que le tintement métallique, m'ont paru coincider avec les circumstances suivantes : 1. Lorsqu'il existe deux ou plussieures communications fistuleuses entre la cavité occupée par l'air et les bronches. 2. Lorsque cette cavité est extrêmement vaste et ne contient qu'une très petite quantité de liquide."—Traité de l'Auscultation Mediate, tome premier, p. 131, Paris, 1826.

[2] Skoda writes thus—"Lannec stellte sich vor, es musse eine Hohle Luft und Flussigkeit Enthalten, um zur Erzeigung der besprochenen Erscheinungen

I can find nothing to account for the complete absence of this phenomenon, except it were due to the largeness of the perforation in the pulmonary pleura. It was certainly a very free opening. It will be noticed, on reference to the record, that the opening was about an inch in length. It is further to be observed that its edges, though ragged, were made up of an elevated, rather firm tissue, so that the perforation must have been constantly kept open, and never have had an opportunity to act in a valve-like manner. At any rate, this very free patency is the only peculiarity about the

tauglich zu sein, und es hat dieser Ansicht meines Wissens noch niemand widersprochen. Ich glaube, dass die flussigkeit dabei ganz uberflussig ist. Ein krug kann ganz trocken sein, oder etwas Flüssigkeit enthalten man bringt die beiden Erscheinungen darin gleich leicht hervor; zur Erzeugung des metallischen echo im zimmer ist keine Flussigkeit nothig. Wenn man in ein mit Luft gefüllten magen angesetzes stethoscop spricht, so ertont innerhalb des magens der metallische klang und auch der amphorische Wiederhall; der Magen moge keinen tropfen flussigkeit enthalten, oder zum Theil mit Wasser gefullt sein.

"Lännec glaubte ferner, die Kaverne, oder die lufterfüllte Pleuralhohle musse nothwendig, mit einem Bronchus kommunicirei, damit darin die amphorische Wiederhall oder der metallische klang durch die stimme erzeugt werden konne. Bei Pneumathorax bleibt, in den seltesten Fallen die kommunication zwischen der Luft in der Pleura und jener in den Bronchi frei, und dennoch findet sich nicht oft ein Pneumathorax, ohne dass man metallischen klang oder amphorischen Wiederhall zuweilen wahrnimmt. In dem eben erwahnten Versuche mit dem magen kommunicirt die Luft im magen ebenfalls nicht mit der Luft im stethoscop, und dennoch gibt die den metallischen klang U. S. W." —Ueber Percussion und Auscultation. Erster Theil. s. 131. Wien. 1844.

communication with the internal air that I can lay my hands upon. And though, in the face of the experiment of Skoda, mentioned above, it is perhaps not fair to assume that any such communication with the external air is necessary to the formation of metallic tinkling, it nevertheless seems to me that the vibration of a valve-like process at the opening into the pleural cavity, or even the vibration of a very thin layer of lung tissue, granting that the pneumothorax and metallic tinkling had continued after the perforation in the pleura had been healed, may be a necessary factor in the production of this peculiar phenomenon. As to the one of the conditions mentioned by Laennec under which he found amphoric buzzing to be present alone—namely, the existence of a large cavity filled with air, and containing very little fluid—there is no doubt but such was present here, except during the first week, and for a very short time afterwards, when the air had got greatly absorbed. To that cause, if it be a cause, may be possibly referred the absence of the phenomenon the day before the death, when the distension was very great.

It will be noticed from the record that the amount of air in the pleural cavity was subject to great variations. I have said incidentally that the air was absorbed, and I do so because I do not well see any other means by which it might get away in such cases. Still, having regard to the size of the aperture in the pleura, and of the bronchus communicating with it, I am not sure but a forcible expiration might, in certain relations between the pleural opening and the bronchus, have expelled a certain amount of the air by the bronchi and trachea. Be this as it may, it did disappear and reappear with very great rapidity; so markedly, that I felt shut up to the conclusion that it was utterly useless to attempt to relieve my patient by tapping, as it was bound to re-accumulate so quickly. I regret, however, that I became so possessed with this idea, because I feel now that, had I tapped the right chest when the left lung began to become so embarrassed the day before death, it might at least have given the poor fellow a certain degree of euthanasia.

It would appear that the fresh pleuritic attack on the right, which came on a day or two before death, however set up, had a considerable amount of influence in precipitating the fatal issue of the case, as it produced fluid, which, accumulating in the pleural cavity, already filled with air, increased the tension on the lung surface, and still more aggravated the consequences of the complete collapse, which had been slowly induced by the gradual undoing of the pleuritic adhesions under the continued action of the air present in the pleura; whilst the imprisoned air at the same time pressed upon the heart and other lung so as to interfere with their action. Along with this came on an attack of congestion through overaction of the left lung, possibly also aggravated by injurious pressure upon it and upon the heart by the air in the right pleura; and thus the scene was closed. The next case of the kind that comes under my care, I certainly will tap, to endeavour to lessen these distressing results of pressure.

As to the treatment. My first case shows that little is necessary. All that was done in his case was to put on a tight bandage and attend to his general health, the pneumothorax being fairly considered as a symptom of some deeper lesion. There seems to me, as I have already stated, to be little doubt but the air in the pleura can be slowly absorbed by some dialytic action of the pleural sac, provided the opening from the lung heals up, and that thus its re-accumulation is prevented. But if it does not become re-absorbed, it appears pretty evident that the opening in the visceral pleura is permanent, and that no good results can be obtained by tapping in a truly therapeutic sense. In case of impending death, however, from the results of pressure in the pleural cavity, I do believe that tapping may give considerable relief to symptoms for a time.

As cure, if it is to be effected, is almost certain to be brought about by a certain amount of pleurisy, and possibly also may lead to pneumonic change as a secondary result, we ought, I think, to be on our guard, so as to prevent, as far as possible, the extension and aggravation of the pleuritic process. Farther than this, I do not see that there is anything that can well be done.

In conclusion, I think, 1st, that, from the occurrence of cases such as these, we are warranted in assuming that, though pneumothorax is usually a complication of serious lung lesion, yet it may occur from very slight causes indeed, and is by no means necessarily a part of tubercular phthisis.

2d. That occurring early, and in a patient otherwise healthy, there is no good reason to give an altogether unfavourable prognosis. Whatever be the nature of the pulmonic lesion, in a certain proportion of such cases, such as the one reported by Dr Thorburn and my first case, the patients get ultimately quite well.

3d. That our opinion upon the probable termination of such early cases should be, therefore, based essentially upon the history and general condition of the patient, as to bulk, absence of cough, night-sweats, etc., etc., and not merely upon the occurrence of this symptom.

But I must bring these remarks to a close, as I feel that I have already trespassed too long upon the time and attention of the Society. My apology for so doing must be my anxiety to lay before them the records of these interesting and rather rare cases, in the hope that by so doing I am adding a little to our present somewhat scanty records of this accident, and possibly may also, at the same time, be contributing to steady the views entertained by members of our profession in regard to it. Should I have succeeded only a little in these efforts, my work and the Society's time will not have been spent altogether in vain.

### ARTICLE X.—"*Shut your Mouth and Save your Life:*" *Being Remarks on Mouth-Breathing, and on some of its Consequences, especially to the Apparatus of Hearing: A Contribution to the Ætiology of Ear-Disease.* By JAMES PATTERSON CASSELLS, M.D., M.R.C.S. Lond.; Fellow of the Faculty of Physicians and Surgeons; Surgeon to, and Lecturer on Aural Surgery at, the Dispensary for Diseases of the Ear, Glasgow.

(*Read before the Glasgow Medico-Chirurgical Society, 5th January* 1877.)

IN this communication it is intended to demonstrate the truth of the proverb that has been chosen as its title, by showing that shut mouths and open nostrils are conducive to good health; and, conversely, that open mouths and shut nostrils are injurious, not only to the general health, but especially to the health of the organs of hearing;—to show, in fact, that neither perfect health nor perfect hearing is compatible with the habit of mouth-breathing; and that to shut the mouth, as the proverb enjoins, is, literally, to save the life.

It is, perhaps, unnecessary to say here, that the subject of mouth-breathing and its evil consequences, considered in a general sanitary sense, have not escaped the notice of previous observers; for, doubtless, most of us remember the quaint and vigorously-written book on this subject by Catlin, the celebrated American traveller; with this exception, however, previous writers on the subject of mouth-breathing have neither been numerous nor influential enough, besides being too ambiguous in their treatment of the question, as well as too loose in their facts, to establish in the minds of others a conviction of the truth of the evils of mouth-breathing; consequently, it has not received that amount of attention which it so justly merits.

Catlin called this mode of breathing "the most destructive of habits," and designated it by the classical name of "malo inferno;" but he, as well as other writers, have considered its evil consequences from a general sanitary aspect alone, perfectly oblivious of the fact, that the power of its evil influence fell chiefly and directly upon the organs of hearing. So far as can be ascertained, no one has hitherto called attention, as is done in this paper, to the causative relation that exists between mouth-breathing and deafness, and to the insidious influence of this habit in the production of the most serious pathological changes in the tissues of the apparatus of hearing; and, in this way, to many of the more serious, because unexpected, complications, and incalculable dangers, that may and do arise in the course of such tissue changes. Therefore, knowing from experience how well established these facts are, one can condone the style of language indulged in by Catlin, when he said, "If I were to endeavour to bequeath to posterity the most important motto which human language can convey, it should be in three words, Shut your mouth."

But it has been said that previous writers on this subject were neither numerous nor influential; and to justify this statement it is sufficient to say, that no reference is made at all to it in general medical literature. Some years ago, indeed, a semi-popular article on mouth breathing appeared in an American Journal,[1] most probably called forth by Catlin's then recently published work. With this exception, and a short paper on the general baneful influence of this mode of breathing,[2] by a Continental writer, no work except Catlin's is extant on the subject. Of Catlin's book it may be said that its style to some extent neutralized its influence; a style that might have appeared ludicrous, indeed, had it not been justified to some extent by the unparalleled experience of the author. Should this paper, therefore, lack the *verve* of that book, such tameness does not arise from a lessened appreciation of the evil consequences of the habit, but from a feeling that this subject ought to be treated with some approach to scientific soberness, in order that its truth may be established.

Apart from the special aspect from which I intend to view this subject, I may be permitted to say that the injunction given by this proverb is full of wise counsel, and were it acted on more generally than it is, many of the anomalous ills that afflict mouth-breathers would disappear; for in a country like ours, "where there is plenty of weather but no climate," and where the rays of the winter's sun too often fail to slant our window panes of a morning, worsted in their struggle to penetrate the almost Cimmerian darkness that envelops some of our cities, it is not possible to overestimate its value in the prevention of bronchial affections alone; while to those who breathe the purest air through a foul cavern, to which a mouthful of carious teeth may be likened, and thus unconsciously injure their health, it would simply prove invaluable. But when this advice is viewed from its physiological aspect in relation to the apparatus of hearing, it becomes replete with interest as a most valuable injunction to all who desire to retain the function of hearing perfect; or to improve it, if it has become impaired. I would also suggest to medical practitioners in general, that this proverb merits their best attention, regarded in a sanitary sense, for I feel satisfied that in their hands it might prove to be a most valuable therapeutic procedure, not only in the prevention and cure of ear-diseases, but in the prevention and cure of those ever-recurring catarrhal affections of the naso-bronchial mucous membranes, which prove so intractable under every and all forms of treatment.

Before passing on to consider the more immediate subject of this paper, let me place before you the following propositions, which may, meantime, be postulated, in proof that man is a nose-breather by nature. Before doing so, however, and as much in your interest as in my own, let me remind you of the rule that Descartes laid down for his own guidance in searching for scientific truth; it is as follows:—"Give unqualified assent to no propositions but those the truth of which is so clear and distinct that they cannot be doubted;"[1] and this rule I ask you to keep constantly before you in considering these propositions, in order that you may either support me in the conclusions to which I have arrived, or destroy the foundation upon which they rest.

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[1] The Atlantic Monthly.

[2] By my friend, Dr Guye of Amsterdam, and which I heard read at the International Medical Congress in Brussels in 1875.

Because that "man had the breath of life breathed into his nostrils," Catlin has asserted that man is a nose-breather by nature—a proposition that is unassailable, you will agree, if in this very scientific age opponents of this conclusion would admit the evidence founded upon; happily there are other facts, as it appears to me, that go to prove, independent of the one quoted, that he is not only a nose-breather by nature, but ought to continue to be one; and as these facts can become the subject of further investigation by those who are disposed to test the validity of this conclusion, I shall state the following propositions:—The newly-born infant can only respire by the nostrils; air inspired by the nostrils is totally different from that inspired by the mouth; disuse of the nasal passages begets abnormal tissue-changes in their structures; and, lastly, nasal breathing is essential to perfect hearing. As some of these statements may appear rather astounding, I shall show, I hope, that if they be so, they are nevertheless well founded.

The first inspiration that the newly-born babe takes is drawn through the nostrils, and if these passages be closed experimentally, the young child cannot breath at all, even though its mouth be held open purposely. I have repeatedly satisfied myself by experiment that this statement is true; several times indeed, in thus experimenting many years ago on newly-born infants, both in those who had and those who had not breathed, I have almost carried the experiment too far, almost to complete arrest of the respiratory act—to complete suffocation, in fact.[2] As the result of these experiments, I emphatically repeat that a newly-born infant breathes at first by the nose only, and continues by preference to breathe through the nostrils till circumstances arise that call forth mouth-breathing.[3] That air enters the nostrils of the infant before entering its mouth, is a well-established fact, for cases are on record (by Mach and Ogston and others) in which air was found in the tympanic cavities of the newly-born infant when no air could be demonstrated to be present in the lung-tissue.[1] That air cannot enter the tympana through the mouth, if the nostrils be closed, I will show on anatomical and experimental grounds before concluding these remarks. That air inspired by the nasal passages is totally different from, and better fitted to enter the lungs than, that inspired by the mouth alone, is, I would venture to assert, the result of the experience of every one who has profited by his experience of life in the murky and fog-laden air of our cities. But a further and physiological fact may be advanced in support of the statement, that the air traversing the nostrils is different from that passing through the mouth; is changed, in fact. This inference rests upon the fact of the difference of the behaviour of the lining membranes of the nasal and mouth cavities respectively, when air is passed over them for a length of time. If any one will experiment on his own person, and note his sensations, he will discover that with prolonged mouth-breathing the lining membrane of this cavity becomes dry and parched, and the act of respiration is at last disagreeable, and may even become painful. While, with prolonged nasal-respiration, not only is there no such distressing or painful sensations as are experienced by mouth-breathers; on the contrary, the act of breathing through the nose is one of entire freedom from such annoyances, and its lining membrane, instead of becoming dry and parched, as did the mucous membrane of the mouth under like circumstances, remains moist, even becoming moister as the respiration of the air is continued. What may be the precise nature of the change thus effected on the air, other than a difference in its temperature and moisture by the respective modes of breathing indicated, is not at all apparent; but the point may be practically tested and conclusively settled by watching the behaviour of mouth-breathers and nose-breathers in one of our city fogs, or in a room the air of which is largely contaminated with the admixture of tobacco-smoke. In either of these circumstances, the coughing and sputtering and spasmodic action of the respiratory muscles of the mouth-breathers are seen to stand out in noisy contrast to the silent and peaceful breathing of those who shut their mouths. That air, therefore, is only fitted to enter the lungs after it has undergone this purifying influence of the nasal passages, surely cannot be disputed.

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[1] Discours de la Méthode pour bien Conduire la Raison et chercher la Vérité dans les Sciences.

[2] My friend, Dr Hugh Miller, Physician-accoucheur to the Glasgow Maternity and Lying-in Hospital, has quite recently, at my suggestion, repeated these experiments, with the result of confirming the conclusion which I have arrived at.

[3] Watch a baby of a few days' old who has "snuffles," and its struggles for breath as well as its difficulty of swallowing; these may convince you that mouth breathing only arises in some dire necessity, and is not the natural mode of respiration.

But there is another fact, of perhaps greater importance than those adduced, to be urged against this mode of breathing, and which has not been recognised by general practitioners, nor even by the specialist,—it is, that the habit of mouth-breathing ultimately unfits the nostrils for the free transit of the air.

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[1] Indeed, so clearly is this fact established, that advantage has been proposed to be taken of it by some medico-legal jurists in cases of suspected infanticide, in which the hydrostatic test fails, as a proof that the infant had breathed.

Basing our inferences on a well-known physiological law, one might expect to find that the changes set up in the tissues of the disused nasal-passages in a case of confirmed mouth-breathing, would be of the nature of atrophic degeneration; and observation shows that this expectation is to a large extent, if not altogether, realised; so I would be disposed to regard the congestions, the ulcerations, and the various forms of hypertrophy of the Schneiderian membrane seen in such cases. Whether or not the origin or predisposing cause of these changes may consist in a loss of local tone through a deficient supply of nerve-force, is at present difficult to determine; but it is no less certain that this habit of breathing, however begun, does aggravate already existing tissue-changes, such as have been mentioned, and may, I am convinced, give rise to them *de novo*. In either supposition, probably by the deprivation of the stimulus of the atmospheric air, so I am led to conclude, from the marked improvement that speedily follows in most cases of non-specific ulceration of the lining membrane of these passages, after a few days' resumption of the natural mode of breathing, without the aid of other measures. While these are but conjectures in regard to the origin of the tissue-changes in the nostrils, the fact yet remains, that mouth-breathing leads to narrowing of the nasal passages, and in many cases to complete closure of them, just as in the somewhat parallel instance of a disused pathway becoming covered over, and at last choked up, by rank, if luxuriant, vegetation.

In regard to the last statement, that nasal respiration is essential to perfect hearing, I have to say that the positive proof of this statement falls to be taken up in the course of this paper; and here, meantime, I shall offer negative evidence only in proof that mouth-breathers are invariably dull hearers, by citing the evidence of an independent observer, viz., that of Catlin. Let me ask you, also, to observe the contrast that is established between civilized life, in which mouth-breathing is the rule, and deafness, and all forms of ear disease, painfully frequent, with that of savage life, in which there is no mouth-breathing, and no defective hearing.

In the course of his travels among the native Indians of America, Catlin visited 150 different tribes, numbering upwards of two millions of individuals, living in a savage state. Among that large number there were found only three or four deaf mutes, and not a single individual who was either dull of hearing or deaf. Nor could any of the chiefs of these tribes, who were all specially interrogated on this point, remember or even find any member of their respective tribes who had been in the slightest degree dull of hearing; and these inquiries extended back over a period of ten years previously to that time at which they were made; a result not to be wondered at, when we learn from the same trustworthy source, that in all these tribes not a mouth-breather was to be seen or found, or was even known. This latter fact is explained by the custom and training of the Indian mothers. From their earliest infancy, Indian babies are trained to breathe by the nostrils alone, never opening their mouths except to take food or to use their tongue.[1]

I shall permit you to draw your own conclusions from this evidence, only remarking again that the further proof that nasal respiration is essential to perfect hearing, will be considered at an advanced stage of our communication.

The recognition of a case of confirmed mouth-breathing is not attended by the slightest difficulty. The retracted lips, the open mouth, and other distortions of the face, the quality of the voice, and the stertorous breathing at all times, point out such an individual at once,—a conclusion which is confirmed, if such be necessary, by an inspection of the interior of the nasal passages, and by holding a mirror immediately below their orifices, and which shows, by its almost undimmed surface, that these canals are nearly, if not quite, unused for respiratory purposes.

In regard to the origin of this habit, it is difficult to say whether it is a cause or a consequence in any given case. In infancy and childhood, when all the tissues of the naso-pharynx so frequently and so readily become the site of acute nasal catarrh, breathing through the mouth in such circumstances becomes a necessity, so long, at least, as the nasal passages are partly or wholly obstructed; but the neglect to treat the nasal affection, or, if treated successfully, to correct this temporary mode of breathing, establishes it as a habit, which becomes a confirmed one, either by continued neglect or repeated attacks of the original nasal affection.

I would remark here, that I am disposed to regard the simple nasal catarrh or "snuffles" of infant life as the result of a change in the mode of breathing from nose to mouth,—a thing all infants, after a few weeks, are prone to do, and to continue doing, unless corrected.

In regard to the treatment of a confirmed case of mouth-breathing, the essential as well as the primary step towards a cure is to break the habit; for difficult as it may be to conquer and to abandon, it can nevertheless be overcome by determined and well-directed efforts on the part of those interested. This primary step consists in shutting the mouth, and, despite the distress that this occasions, keeping it shut by a strong effort of the will. Simply closing the mouth, as is here directed, several times a day, at first for short, and afterwards for longer periods, especially when walking in the open air, cannot fail, after a time, in re-establishing nasal respiration. By-and-by, the pure cold air, at first respired with some difficulty through the hitherto disused nasal passages, acts in lessening the congestion and tumefaction of these canals, which at last allow of the act of respiration being carried on without distress or difficulty. When this result does not follow speedily, it may then be necessary to treat in a more formal manner the local defect in the nasal passages; nothing, however, need be said here on this point, as the principles of our art suffice to guide us in the selection of a remedy suited to the nature of the obstruction; but "principiis obsta, sero medicina periatur, cum mala pertingas invulnere moras," which is best paraphrased by the proverb that at present sounds the keynote of the modern practice of medicine and surgery, viz., that "prevention is better than cure."

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[1] These are striking facts; and, descending in the scale of creation, we find that the brutes seldom or never become deaf, except from the decay of advanced age; they are also nose-breathers.

While holding to the opinion that nasal breathing is the natural mode of respiring, and that mouth-breathing seldom becomes a habit in the infant till it has existed as a necessity, I, nevertheless, cannot divest myself of the influence of the fact, that nature is prone to transmit to the offspring those traits and habits that have originated and developed themselves in the parents; therefore I can conceive of the possibility, by a process of development, of the creation of a race of mouth-breathers. Still, were this proved, and not a mere speculation, I would say in all cases, correct the habit; for, as our bodies are at present constituted, this mode of breathing, however instituted, is not conducive to a healthy state either of mind or body. As a fact of experience, we find that at all ages, whether sleeping or waking, mouth-breathers are the most miserable, because the most unhealthy, of people; and if we conceive of individuals such as I have indicated becoming the progenitors of the future generations of men, it does not demand that the prophetic vision be strained to see in the not distant future, in the ordinary course of nature, sons of these sires a race of dull-brained, badly-formed, and imperfectly developed mouth-breathing creatures, peopling a province or even a country.[1]

If the general *malaise*, and the other *désagréments* referred to, were the sole effects of this mode of breathing, it would still be desirable to correct it; but, unhappily, these are not the only or the worst consequences of this habit; there are others of greater importance in relation to the apparatus of hearing, that makes it a matter of vital importance that this pernicious practice be overcome and finally abandoned; and we now proceed to inquire into the nature of this relation.

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[1] *Verb. sap.* If you find the baby's mouth open during sleep, close it; for the sleep of the mouth-breather, whether infant or adult, is restless and unrefreshing, because unrestoring. And need I point out the consequences that can ensue to the human organism from the continued deprivation of sound refreshing sleep, "the loveliest, since it dreams the least"! Such sleep as this seldom visits the mouth-breather, seldom visits him at least in its true character, as "life's nurse sent from heaven to create us anew day by day:" with consequences to him not difficult to imagine.

In order to show the relationship that subsists between mouth-breathing in its confirmed form, and disease of the apparatus of hearing, as well as the sense of hearing itself, it is necessary to state, in somewhat general terms, that the integrity of this apparatus, and therefore of the sense of hearing, is only maintainable by a certain degree of intratympanic tension, which, under all circumstances, bears a constant relation to the air-pressure on the outside of the membrana tympani, and that any, even the slightest disturbance of this normal degree of intra-tympanic tension is followed by defect of function, and, if long continued, by tissue-changes in the apparatus of hearing. Now as this normal degree of intra-tympanic tension, so necessary to the preservation and economy of the organs of hearing, is only possible when the nostrils are open, and nasal respiration carried on, and, on the other hand, disturbed at once, in a greater or lesser degree, by any narrowing or closure, of these canals—a condition which frequently precedes, and is always aggravated by, mouth-breathing—it becomes apparent that the cure of this habit is an important matter on other than purely æsthetic or sanatory grounds.

But it may be asked, What evidence is there that warrants the assumption of this hypothesis of a normal degree of intra-tympanic tension as necessary for the maintenance of the integrity of the tissues of the ears and normal function? and further, What is the evidence that supports the statement, that this normal degree of tension is only possible while the nostrils are open, and disturbed so soon as they become narrowed or closed, and mouth-breathing as a habit established? Before bringing forward this evidence, it seems necessary to recall briefly to one's mind the rôle which the act of deglutition plays in ventilating the tympanic cavities, and in adjusting the normal intra-tympanic tension when it has become suddenly disturbed; and also, incidentally, how this same proceeding (the act of deglutition), under other circumstances, still in itself acting normally, becomes an active power, not in ventilating the ears nor in adjusting their tension, but in causing a disturbance, already begun in their tympanic tension, to intensify itself.

With the completion of the first stage in the act of swallowing, the nasal passages and the upper pharyngeal space are almost completely shut off from the pharynx. In the second stage of this act the pharyngeal constrictors close by reflex action on the morsel of food or saliva, and force it onwards in its course to the stomach. With this contraction of the pharyngeal muscles, however, there takes place also a contraction of those muscles that preside over the Eustachian canals—the tube-muscles—whereby these canals are more or less opened, and the tympana are ventilated; in other words, every act of swallowing is accompanied by a contraction of the tube-muscles that opens the Eustachian canals, permitting of the free ingress and egress of the external air to the tympanic cavities.

This act of ventilation not only takes place under ordinary states of atmospheric pressure, but sudden and violent changes in its density, which might do serious injury to the ears, are obviated by the involuntary act of swallowing on our part, and the unconscious adjustment, so far as we are concerned, of the tension of the apparatus of hearing in these altered circumstances.[1] Besides this extraordinary ventilation of the tympana, which, by the way, I regard as a reflex action as distinct in character as are the contractions of the pharyngeal muscles on a morsel of food, there is the ordinary unceasing interchange of air to and fro by the Eustachian canals, whereby, for the ordinary purposes of audition, the normal balance of tension is kept uniform. Now, the position of the tube-orifices on the lateral aspects of the naso-pharyngeal cavity, in relation to the posterior openings of the nasal canals and that of the mouth, leaves us in no doubt, even were this not capable of further proof, that the air for the purposes of ventilation of the ears can pass along the nostrils only, and that when these are in any way narrowed or closed so as to hinder or altogether to obstruct its free passage, the normal ventilation of the tympana becomes impossible.[2] As already stated, the sole effect of swallowing with open nostrils is to ventilate in a more perfect manner the ear cavities ; but so soon as the nostrils become narrowed or closed, every act of swallowing reacts upon the tympana and the apparatus of hearing in the following way : The contraction of the pharyngeal constrictors, acting like an exhaust-pump, withdraws the air from the naso-pharyngeal cavity, and by-and-by from the tympana also. In this manner the quantity of air in the tympana is lessened, *pari-passu*, with the tension of these cavities. As no new supply of air reaches these cavities, or can reach them, otherwise than under peculiar circumstances, so long as the nostrils are in the state here supposed, it follows that serious changes, both in regard to function and tissue, may take place, and do indeed occur, as clinical observation shows conclusively.

That air passes into the cavities of the middle ear to and fro, as above indicated, with every act of respiration through the nostrils when the mouth is closed, and cannot do so when these circumstances are reversed (*i.e.*, when mouth-breathing is temporarily instituted with closed nostrils), is proved in those cases in which the membrana tympani is abnormally thin ; here, in the circumstances first supposed, the membrane may be seen to move outwards and inwards simultaneously with the respiratory motions, but this phenomenon is only met with when the respiration is nasal and the Eustachian tube quite normal; it ceases at once on closing the nostrils and breathing by the mouth alone.

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[1] I have frequently experimented on split-palate cases, and proved what I say in the text.

[2] Left-ear deafness, so long a mystery as to its origin, is explained by the fact that the ventilation of this ear is always more liable to disturbance, owing to the natural narrowness of the left nasal passages, and to their becoming more readily occluded by a congestion of their tissues as compared with the right nares, which, in 80 per cent. of the patients afflicted with deafness, is wider and roomier than that of the left side. But when, as happens now and again, we have a case of decided right-side deafness, with the left unaffected, we find invariably that the right nares are narrower than the left, which in such cases are roomy beyond ordinary experience.

In proof of the statements already advanced—viz., that a certain degree of intra-tympanic tension is necessary for perfect hearing, that disturbance of this normal degree of tension is at once followed by impaired function and tissue change, that this disturbance most readily arises from non-nasal respiration (or mouth-breathing), and that it is only maintainable by open nostrils and nasal breathing—let the following simple and convincing experiment be performed, by one, *whose ears and the whole apparatus of hearing are perfect*, upon himself. With the forefinger and thumb close the anterior orifices of the nasal passages, and while they are thus closed, perform the act of respiration by the mouth. Now and again, quite involuntarily, the saliva, collecting in the cavity of the mouth, finds its way to the pharynx, and causes its muscles to contract. This act of swallowing, repeated several times, calls forth a series of sensations that did not exist previously to the institution of the experiment, nor till the act of deglutition had occurred once or twice in rapid succession. The sensations of which the experimenter is himself conscious are as follows:—In a greater or less degree, a feeling of tightness in the ears, perhaps a little vertigo, and a varying amount of subjective aural-tinnitus, in addition to which there is very considerable dulness of hearing, especially of articulate sounds. It may now have been noted that the simple act of respiration through the mouth, with the nostrils closed, did not cause any of the sensations just named, but so soon as the pharyngeal muscles contracted several times in rapid succession, they were at once experienced in varying degrees of intensity, and that they continued so long as the cause or conditions that called them into existence continued to act; removing simply the obstruction that closed the nostrils, and swallowing the saliva several times, caused these symptoms to vanish as rapidly as they were created. On the other hand, the act of deglutition performed with *open* nostrils fails to produce any appreciable effect on the ear other than the natural ventilation of the tympanum. Certainly it fails to call forth the train of phenomena above described as the result of the act of swallowing with the nostrils *closed*.

This experiment, apart from the object of proof for which it was undertaken, enables the experimenter to form his own estimate of the painful consequences that arise out of a nasal catarrh which closes the nostrils so that breathing through them is no longer possible; while it enables him to realize somewhat of the nature of the affliction under which mouth-breathers may suffer in a greater or less degree for a whole lifetime perhaps.

In the above detailed experiment it is hardly possible to deny that all the subjective phenomena were the direct result of closing the nostrils; but it may be asked if it be a warrantable inference to say that they were the expression of disturbed tympanic-tension. That this is a just and a true inference is susceptible of proof as follows; meantime, it may be helpful to the better comprehension of this proof if I say that, in the above experiment, the air in the tympanum was sucked out by the sucker-like action of the contracting pharyngeal muscles in the act of deglutition, creating thus a disturbance in the tympanic tension, and calling forth the subjective sensations that were experienced, and which again passed away so soon as the tympanic tension was restored by the act of swallowing, while the nostrils were no longer closed; in this way giving entrance to air which passed into the tympanum.

That these symptoms were the expression of disturbed tension, and that this disturbance was the direct result of diminishing the air in the tympanum, as in the above experiment, is proved as follows:—Let the membrana tympani be inspected ocularly, and its movements noted while this experiment is in process of execution; it will be seen to remain unaffected by the act of respiration by the mouth, although the nostrils be closed, but so soon as swallowing takes place in these circumstances, this membrane becomes more concave than normal, and congested as well, this concavity and congestion increasing with each act of swallowing. Simultaneously with the first movement of this membrane inwards all the symptoms above noted are called forth in their order, and these intensify themselves as the curvature of the membrane inwards increases, which it does with each additional contraction of the pharyngeal muscles. If now the membrane be still kept under observation, and the act of swallowing be performed with the nostrils free from all obstruction, it may be seen with each such act to regain gradually its normal curvature, sometimes to regain it at a single bound; while at the same time, and in an equally gradual or sudden manner, the subjective sensations and the congestion of the membrane pass away. Thus it is seen that the changes in the curvature of the membrana tympani coincide, and are simultaneous in their production and removal with the production and removal of the subjective phenomena, and that these were created solely by the act of swallowing with the nasal passages closed.

The fact that an immediate change in the abnormal curvature of the membrane, and a return to its normal concavity, together with relief from the disagreeable symptoms so frequently mentioned, take place at once on the act of swallowing with the nostrils open, proves also the absolute need of unobstructed nostrils and nasal breathing as the chief, if not the sole, condition for the maintenance of the normal intra-tympanic tension, and therefore of perfect hearing.

It is here that the causative relation that exists between the confirmed habit of mouth-breathing and ear-disease is seen clearly, because nasal respiration in such circumstances is no longer possible, as has been already shown; and here it has been established that nasal respiration is essential to perfect hearing, as well as to the integrity of the tissues of the organs of hearing.

These facts in proof are themselves proved to be real by fixing in the meatus a manometer, the column of mercury in the instrument by its movements indicating the variations in the density of the intra-tympanic air, and therefore the changes that take place in the curvature of the membrana tympani. As an evidence that the external column of air is an important factor in the causation of the subjective symptoms already noted, when the intra-tympanic air is rendered denser or rarer as in the experiment, it is only necessary to close hermetically the outer orifice of the external meatus of one ear before the experiment is begun, and after it is being carried out. With each act of swallowing all the subjective sensations are called forth in the unprotected ear, while in the other, if the meatus, before and during the experiment, be hermetically occluded, not the faintest sensation is experienced.

Now, while this contrast is being noted, let the obstruction to the entrance of the air into the closed meatus be removed; in an instant these subjective sensations are felt in it also with equal intensity to those felt in the first instance in the other and unclosed ear; these passing away equally, as in the former experiment, on the readmission of air into the tympanic cavities. If instead of occluding the meatus, as in last experiment, with the finger-tip, we close it as effectually with an instrument devised to permit of the membrane being ocularly examined while the air is excluded from the canal,[1] it can be seen that, so long as the air is excluded from the meatus, no movement takes place in the membrana tympani thus protected, and therefore none of the subjective sensations indicative of disturbed tension are felt. But on the instant that the air is admitted to exercise its influence upon the external surface of the membrana tympani, that structure is seen to move suddenly towards the tympanic cavity, and, simultaneously with this movement, the sensations already described are at once felt.

The evidence here given in support of the statements, that a certain degree of intra-tympanic tension is necessary for the perfect function of the apparatus of hearing, and that open nostrils and nasal respiration are essential factors in the maintenance of this normal balance of tension between the intra- and extra-tympanic air-pressures, is considered sufficient and conclusive; but were it otherwise, and necessary, further evidence could be adduced in proof that the hypothesis here briefly referred to in general terms is a real and a thinkable one; because not only does it include and explain all the facts of audition, but it explains much that pertains to aural pathology, and affords a firm foundation on which to build a rational theory of the Ætiology and Evolution of ear-disease.

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[1] Siegle's speculum does very well, if carefully fitted to the orifice of the meatus.

Coming now to consider the relation of mouth-breathing to the causation of ear-disease, we see that when the nostrils are wholly or in part closed, from whatever cause—a state of matters always existing in those who are mouth-breathers—the same series of phenomena take place in a greater or less degree, and with varying rates of rapidity, as were created by the preceding experiment, with this marked difference, that while in the case of the experiment the sensations called forth are relievable by an act of the will on the part of the experimenter, the symptoms instituted by the habit of mouth-breathing are not removable at all in this way, but in many cases continue, despite all treatment, while more serious pathological changes are set up in the tissues of the organs themselves.

Some of these changes may be very briefly referred to here:—Thus, if the normal intra-tympanic tension be disturbed in any way, besides calling forth at once dulness of hearing, giddiness, aural tinnitus, etc., the following changes take place after a time in the tissues, if the disturbing cause remains unremoved: the membrana tympani becomes more and more concave after a time, and its vessels become congested; its fibrous laminæ become atrophied by the continuous one-sided pressure, and it finally collapses in its entire extent; at the same time the ossicular chain, suffering from the same cause, and sharing in the evils accruing to the membrana tympani, become at last immovable, from stiffening of their delicate articulations and impaction of the base of the stapes. The tendon and ligament of the tensor tympani, relaxed at first by the changes above described, in time contract, so that were it possible, which it seldom is, to remove the other consequences of altered tension, this contracted tendon and ligament mars the best efforts of the practitioner to effect an improvement. In many cases this series of changes may be all the physical tissue-changes recognisable, the characteristic symptoms being gradually increasing deafness and distressing tinnitus, which diminishes or altogether passes away as the deafness deepens. Or a different, and in some respects more serious, set of changes are instituted; thus, in acute nasal catarrh the congestion of the naso-pharynx, by continuity of tissue, invades the Eustachean tubes, which in consequence become concentrically closed; in such a case the ventilation of the tympana, already impaired by the habit of mouth-breathing, is at once arrested, and the following further changes ensue: the air shut up in the tympanic cavities is speedily disposed of, and passive congestion and engorgement of the lining membrane of this cavity follows as a result of the diminished pressure. If this state be unrelieved by art, nature steps in to supply the need. From the highly-engorged membrane a free serous transudation takes place that speedily fills the tympanum, giving relief, to some extent, for a time to the more pressing symptoms.

The issues of such a case are various—indeed its possibilities and its dangers are incalculable—for the more acute symptoms subsiding, and the absorbable portions of the effused fluid becoming removed, there comes an improvement in the hearing power, but never a complete restoration of it, so long at least as the habit of mouth-breathing is continued: or the unabsorbed portions of the effusion becoming organized, and forming true and false bands of adhesion that bind the structures to each other, may effectually hinder even this temporary and limited restoration of the hearing. In many cases, again, the lack of conservative remedial measures at the fitting time allows of a rude form of natural cure taking place; in such circumstances the membrana tympani, "giving way" before the imprisoned fluid in the tympanic cavity, an otorrhœa is established, of which neither the consequences nor the end can be foreseen.

In regard to the treatment of cases of dulness of hearing, or other more serious tissue-changes in the apparatus of hearing due to this mode of breathing, there is much difficulty; each case, after a careful study of its possibilities, and of its impossibilities as well, must be decided on its merits. If the disturbance of tension has existed for a length of time sufficient to give rise to some of the changes that I have described, no treatment may suffice to relieve the symptoms most of all complained of by the patient, or to avert more serious issues. Nay, the nostrils may be made to re-fulfil their office, the Eustachean tubes made patent, and the injunction to shut the mouth strictly followed; and, nevertheless, all may fail to afford the smallest improvement in the hearing, or even to arrest the progress of the tissue-changes that must inevitably end in deafness more or less complete.

Such are some of the changes that mouth-breathing can institute in the tissues of the apparatus of hearing—consequences, I may say, that are to a large extent preventible, if all classes of society, at all ages, rich and poor alike, would shut their mouths and breathe through their noses; and thus, saving their lives, prove that the proverb which has been chosen for a title is true.

## AS TO THE PRACTICALLY ABSOLUTE SAFETY OF PROFOUNDLY INDUCED ANÆSTHESIA IN CHILDBIRTH AS COMPARED WITH ITS EMPLOYMENT IN GENERAL SURGERY. (Note from DR STORER.)

THE writer has become aware, since the issue of the January number of this *Journal*, of the existence of an important typographical error in his article "On the Importance of the Uterine Ebb as a Factor in Pelvic Surgery;" and though the context must have satisfied every reader, save the most cursory, of the real point that was made, he is still desirous that there should exist no possible misconception.

It was stated that the menstrual rhythm, or its polarity, as might almost be said, had much more to do with success in pelvic surgery than had hitherto been supposed, and that, when properly appreciated, it might be taken advantage of. Incidentally, reference was made to the time of occurrence of normal labour—at the tenth menstrual period—and to the practically absolute safety of even very profound anaesthesia during childbirth. Hereby it was not intended to open up any collateral questions, —such as the chance of post-partum hæmorrhage, and the relative tendencies towards this of ether and chloroform, etc., etc., upon some of which, however, the writer believes there are points unappreciated or even undiscussed,—but simply to state the general fact alluded to above. The idea in the writer's mind, and which for many years he has been accustomed to teach in America, was, that *even profound anæsthesia may be borne during childbirth with* ABSOLUTE *safety as compared with the severer cases of general surgery, whether these latter are of necessity or election, and that, were there dangers, they would necessarily be of an almost totally different class.*

His manuscript, as presented to the American Gynaecological Society at New York in September last, and as sent subsequently to the printers of the *Edinburgh Journal*, read—in the attempt to make the paper as concise as possible—"An anæsthetic may be more easily borne by women during childbirth than at many other times."

In correcting the proof, here in London, the following verbal change was made, in the hope of making the precise meaning more evident:—"Even if profound, anæsthesia may be borne during childbirth without danger."

As the article finally appeared in print, it ran:—"Anaesthesia, even if performed without danger, may be borne during childbirth."[1]

This error, otherwise merely ludicrous, became of importance, since it was stated in the context that the writer "first clearly pointed out" the general fact italicized above, and that he moreover stated, in a footnote that, to the best of his belief, it was at the time unknown to, or unappreciated by, Sir James Simpson, of whose collected works it was his great good fortune, together with Dr Priestley, to be selected as editor.

The second point endeavoured to be conveyed was, therefore, the following:—that Sir James, while employing, when necessary, profound anaesthesia in childbed, though in ordinary cases he deprecated more than a slight degree of it, and "though (although improbably) he may have suspected the fact" indicated above, "had not arrived at its full explanation, and no clue was given to it in his 'Memoirs and Contributions,'" as edited by Priestley and

[1] The Printer is solely responsible for this blunder.—*Ed. M. J.*

Storer, "under Simpson's direct supervision;" or even in the posthumous edition of the papers upon Anæsthesia, etc., revised by his son, Sir Walter G. Simpson.[1]

To the profession generally in America, and to many physicians and surgeons in Great Britain whose personal acquaintance the writer enjoys, the explanation now given would have seemed superfluous. There may be those, however, who have wondered at his language as it appeared in the January number of this Journal. The writer has often been laughed at at home for his egotism, arrogance, pluck, or simplicity, as his various critics have phrased it, in confessing that whatever he may have hoped to become in the profession would have been owing to Edinburgh, and especially to Simpson, who first truly taught him what to observe, how to observe, and what to do with the observations made. It would have been a sorry thing, after Boston's attempt to steal his laurels from Simpson's dying brow, for the Bostonian who had so persistently endeavoured through all these twenty-two years to secure him simple justice, to have seemed, and on Scotch ground too, to detract a hair's weight from his work and his memory.

# Part Second.

## REVIEWS.

### *Kirkes' Handbook of Physiology.* By W. MORRANT BAKER, F.R.C.S. Ninth Edition.

THE appearance of this, the ninth edition of Kirkes' Physiology, will be welcomed by many students of medicine. Though, as the author remarks, the average length of life of new physiological facts may be reckoned at about three years, still we propose to call attention to the omission of some facts whose period of existence has been longer than that indicated. We would also venture to make some suggestions which, we trust, may be of value to the author, should he be called upon for a tenth edition.

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[1] It might, perhaps, be thought that Simpson's remarks upon menstruation as a kind of vicarious respiration (Obst. Mem. and Contrib., Scotch edition, p. 20; and in identical language in the American edition, though, perhaps, with differently numbered paging); and upon the periodical asthma of amenorrhœa (loc. cit., and No. XXXVII. of the "Clinical Lectures on Diseases of Women," first published in the London Medical Times and Gazette, 1861, reprinted in America in 1863, and revised subsequently to Simpson's death by his nephew, Professor Alexander R. Simpson of Edinburgh), might indirectly have furnished a "clue" to the result that the writer has attained. This view, he thinks, cannot fairly be held, though he thinks that in this, as in many other matters, it was Simpson's remarks upon just such collateral subjects that, as is sometimes said in his country, "set himself a-thinking."

The first chapter treats of the general and distinctive characters of living beings, and the second of the relation of life to other forces. Regarding this latter chapter, we would seriously enjoin the student *not* to peruse it, lest any distinct notion he may have of the definition of such terms as energy, force, etc., may be completely perverted. A short account is given of the chemical composition of the human body, following the method used by Hermann. This way of putting it is always uninteresting to the student.

A short account is given of the elementary tissues of the body, and the localities where each kind of tissue occurs are indicated.

Some of the woodcuts are not quite what might have been expected in the *ninth* edition of such a book. We need only refer the reader to Fig. 34 of hyaline cartilage to justify this statement. In speaking of the changes that may occur in cartilage, calcification is noted, but an equally common change, viz. the appearance of particles of fat within the cartilage corpuscles, is omitted. Further, we defy any student, or even any one acquainted with the subject, to obtain any exact idea of the appearance of white fibro-cartilage from Fig. 37.

The use of the word "molecule," as applied to the fine granules of the blood, we object to, for already that word has a distinct significance in physical science; and even though used by Gulliver for the finely granular basis of the chyle, that is no justification for continuing the use of the word in this connection. A concise and useful account of the discovery of the circulation will be found of value, for the student of the present day, amongst the multiplicity of facts and subjects which he has to get into his head, is too apt to forget the historical side of the subject.

The cause of the second sound of the heart is ascribed to the closure of the semilunar valves. This is not so. It is due to their stretching. In this important matter no reference is made to Ceradini, who worked out this subject most thoroughly. Indeed, from the statements made, the author seems to be quite ignorant of Ceradini's work. In speaking of the cause of the first sound of the heart, the names of Ludwig and Dogiel, and their experiment to prove that the contraction of the muscular fibres of the heart is largely concerned in its production, are omitted.

No mention is made of the effects of poisons, *e.g.* atropine, on the heart and its nerves; and all the manifold considerations about blood-pressure cannot be said to be very well done.

Due prominence is given to Hunter's experiments on the arteries. The description of Fick's spring manometer is very imperfect, for the blood is separated from the alcohol within the curved spring by means of a membrane. The latest and most useful form of this instrument is entirely omitted, although it appeared long before this edition came out.

We expected, after the elaborate and precise experiments of

Ludwig, Owsjannikow, and Dittmar, that the position of the vaso-motor centre in the medulla would be accurately indicated, but only a vague statement is given.

Another historical point duly indicated is of value, viz., the discovery of the diapedesis of the blood-corpuscles by Addison and Waller, a fact too apt to be neglected in the universal mania to ascribe nearly all the recent facts in physiology to some German or other. It would indeed be difficult for any one to apprehend the meaning of the term "vital capillary force."

It is said that the tension of the arterial blood is increased during *inspiration*. This is ascribed to Burdon-Sanderson. Now, we are quite unaware that Dr B. S. said anything of the sort, for just the reverse is the case.

In the histology of the lung the existence of the lymphatic tissue in the bronchial walls is omitted. A better diagram of the submaxillary gland than that of Kölliker could easily have been made. The innervation of the salivary glands is imperfectly done, the mode of paralyzing the secretory fibres *by atropine*, without interfering with the functions of the purely vaso-motor ones, being entirely omitted.

Pancreatine is indicated as if it were a simple substance. Probably it has no existence as such; and if it exists at all, it is complex.

In the test for bile pigments, nitric acid is recommended. Now, it must be *impure* $HNO_3$, *i.e.*, containing *nitrous acid*. The act of defæcation is very imperfectly described.

The changes which the lymph undergoes in passing through lymphatic glands might have been more fully given. The effects of influence of the nervous system on the spleen is omitted. Fig. 188, of a mucous gland, is extremely poor; especially misleading is the appearance of the plug of mucus at its orifice.

In estimating the amount of solid matter in urine, the formula, viz. Christison's, is wrongly given: the multiplicator is not 2, but 2·33. This is a very important matter, and such an error would lead to a very erroneous estimate of the solid matter in the urine.

The tables noting the income and expenditure of the body (terms borrowed from Hermann) will be found very useful.

It would have been better to have maintained the same spelling of the word "Reflection" throughout the text.

The immense subject of reflex action ought certainly to have been more fully discussed.

In speaking of the structure of the cerebellum, the description does not correspond to the drawing, for in the drawing *two* central processes from Purkinje's cells are indicated, while only one really exists. The section of the cerebrum is one of the most miserable and imperfect in the book. More might easily have been made of the value of curara.

The chapter on the physiology of the electrical condition of nerve and muscle is one of the poorest in the book, and in a department bordering so intimately on that of physics, might easily have been more fully developed.

The convoluted urinary tubules are not lined by protoplasm, mapped out into areas, as indicated by Ludwig. The peculiarity of the structure of the renal artery, in containing longitudinal muscular fibres, is omitted. All mention of the researches of Ludwig and Zuwarykin on the lymphatics of the kidney is omitted; so are Englemann's on the ureter. The whole physiology of the secretion of urine is very imperfect, the elaborate researches of Ludwig and his scholars being practically omitted. Chapters on development close the work.

We have endeavoured to point out some imperfections, and offered some suggestions which, we trust, will be found serviceable.

This book, however, has always been conspicuous on account of the fluent style in which it is written; and the student, notwithstanding the above detractions, will find it a book of considerable service to him.

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### *A Descriptive Catalogue of the New Sydenham Society's Atlas of Portraits of Diseases of the Skin.* Compiled at the request of the Council by JONATHAN HUTCHINSON, F.R.C.S. Part II.

THIS second part of the Catalogue describes Plates XXIV. to XLIV. inclusive. The descriptions, cases, and plates are well given; the treatment is not very full, and sometimes we have a hint at the treatment of the class, as well as the individual case. Four small plates are given in the Catalogue of the itch-mite, and several other insects which attack the skin of ox, horse, etc., which give rise to great irritation when they find their way to the human skin. At page 135 we have a woodcut of a vertical section through a purpura patch, showing its plugged capillaries taken from the case of purpura thrombotica, represented in Plate XXXIX.

There is one good feature in some of the cases described. Take that of Addison's Keloid, page 160. In it we have notes, etc., of a rare skin disease, which has been accurately described by the observers, under whose care the patient had been at various stages of the case. This is therefore a valuable contribution to medicine. Any one with the requisite time may, with permission from the several possessors of the drawings of this case, study it continuously—we might almost say clinically—with the aid of the description here given, as well as others which have been published. But who has the requisite time, etc., for such a study?

The present writer describes the condition of things corresponding to the time of the portrait, reports for us from the drawings and descriptions of others who have had the case in hand, compares these with his own delineation and description, and gives us the benefit of his study in so many words. Doubtless, from such a study, our writer is quite familiar with all the various phases of this protracted and interesting case; and it must be a case of very great interest to him from having such a history in delineation, which no mere description can approach in accuracy, or equal in its lasting impression on the memory of the privileged.

But what for the rest of us? we must be content with one portrait at a late stage of the case, and an epitome of all that has been written about it, and with this we are expected to have a vivid recollection of the case. If we had the plates corresponding to the stages which have been described by various writers, as we have their descriptions, we might consider that all of us had rather an intimate acquaintance with this case, its appearances and nature.

Cases thus given to the profession would lay us all under growing obligations to the New Sydenham Society as the plates annually reached its subscribers.

## *A Directory for the Dissection of the Human Body.*

By J. CLELAND, M.D., F.R.S.

THE distinguished Professor of Anatomy in Galway has done good service by the publication of this small volume. Every student of anatomy working in the dissecting-room must have often felt the want of a short, concise, and handy guide to his work. This he will find in the "Directory." The directions for the use of instruments are well put, but we rather object to the use of the word "vivisector" at p. 2, especially in these days of hyper-aesthetic public opinion. The more elegant and less repulsive word "physiologist," or, may be, "surgeon," would have answered the purpose perfectly. The order of dissection recommended will be found to be that which is certainly the best. Two especial features we would cordially recommend to the attention of the student, viz.:—the methods for the dissection of the heart and eyeball,—parts which are only too frequently omitted, and with which the student, often well versed in the arrangement of the muscles, nerves, etc., and their connections, has often a most imperfect acquaintance. This need not, and ought not, to be so, when a bullock's heart and eye can put him at once in possession of the requisite material. With these at his disposal, and a careful use of the text, he may easily acquire a correct knowledge of these too often neglected organs.

The great merit of this book is that it is *thoroughly practical*, and one sees at once that it is written by one thoroughly conversant with his subject, and the difficulties of a beginner in the dissecting-room. The directions are short, not overloaded with facts, and can easily be read over before beginning a part, so that in a short time a working plan of the proposed method of procedure is brought clearly before the student's mind. We cordially recommend every dissector to provide himself at once with this *short but invaluable* "Directory."

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION LVI.—MEETING III.

*Wednesday, 3d January* 1877.—Dr J. D. GILLESPIE, *President, in the Chair.*

I. *Mr Bell* showed a LITTLE GIRL, who, two months ago, had met with an accident of great interest. While playing at the top of a stair, she fell down to the bottom—a distance of 40 feet. Fortunately for her, there was a railing in a recess at the foot of the stair; and on one of the spikes of it she was caught, and, turning over, hung suspended "like a leg of mutton," as her father described it. She was immediately seen by Drs Young and Cuthbert, who, with great presence of mind, carefully protected the wound with lint steeped in carbolic oil. On examination at the Infirmary, it was found that the spike had entered the popliteal space, but without injury to the bloodvessels; split the bone, and passed out again on the outer side. In this way the knee-joint was opened, so that he could pass his finger in. The whole wound was washed thoroughly with carbolic acid, 1-20; the edges brought together as well as possible, and free drainage, along with all antiseptic precautions, used. The highest temperature she ever had was 101° F. The result was, as they could see, that the child had a movable joint and could walk well, any stiffness being merely muscular. Ten years ago, he would have amputated without hesitation. He had certainly never seen a worse case saved. The only other wound she sustained was a slight cut on the head.

II. *Mr Chiene* showed a man admitted into Mr Lister's wards in the summer, for a RUPTURE OF THE LIGAMENTUM PATELLÆ. For particulars, see page 708.

III. *Dr Wyllie* showed a new form of MICROTOME adapted for freezing. It was a modification of the freezing microtome of Professor Rutherford, and was the invention of Mr Fyffe, one of the assistants in the Pathological Department of the Royal Infirmary. Its chief advantages were, *first*, that, owing to the comparatively small size of the well, freezing was effected by it very rapidly; *second*, that the box for the freezing mixture, instead of projecting at the side of the cutting-stage, was placed behind it, and was thus out of the way of the hand; and, *third*, that the cost of the instrument was only 10s.

IV. *Dr Roberts* showed the FANG OF A TOOTH with a pellet of shot embedded in it. The young lady from whom he had obtained this had been accidentally shot some five or six years ago. From that time the tooth had gradually decayed until now, when it had come away. The lady informed him that there were still a few pellets *somewhere* else. The locality was ambiguous, and reminded him of a lady who was described as, while sitting on the grass in India, being *stung* in the "bustle," only the "bustle" wasn't on!

V. *Dr Jamieson* then read a paper on the OIL OF TURPENTINE IN THE TREATMENT OF SCIATICA.

*The President* would be glad to hear any remarks on Dr Jamieson's interesting paper.

*Dr James Young* had been much interested in Dr Jamieson's paper. He had used turpentine in cases of sciatica, especially after the opinion pronounced by the late Warburton Begbie on the therapeutic virtues of that drug. In half-drachm and drachm doses he had found it of no effect. But he would again try it in the way recommended by Dr Jamieson. The best treatment, so far as he knew, was by iodide of potash pushed to iodism. This was often very effectual in relieving pain. He had found good results—the best results indeed—by the use of acupuncture. In every case, of course, it was important to remember that they had to consider the constitutional state of the patient. In the case of one lady he had found turpentine in drachm doses of no effect; but she was much benefited by the potash in six grain doses thrice daily; and after a three months' residence in England the pain never returned. He could not say whether the iodide or change of air had been the cause of cure, but probably both had been of advantage.

*Dr C. Muirhead*, during the last week or two, had in his wards at the Infirmary three cases of sciatica; and as Dr Jamieson had been present there, and suggested the turpentine cure, trial was made as follows:—In the first case two drachms of turpentine were administered every second night on three occasions, but no castor-oil was given along with it. No effect followed. In Glasgow Infirmary, where the patient had been previously, almost every remedy except turpentine had been tried. He next injected chloroform in five-minim doses, thus relieving the pain almost immediately, but producing a feeling of numbness not yet gone. He had also in this case used iodide of potassium in twenty-grain doses thrice daily, on the ground that there may have been thickening of the nerve-sheath at the sciatic foramen. The use of acupuncture needles gave him most relief; but he believed the case was one where the sciatica would continue during the man's life. In the second case, a blister was applied over the tendo-Achillis. He had found it of most advantage when applied in this situation, as it was nearer the branches of the nerve than when applied over the gluteus maximus. In the third case he had used the needles and iodide of potassium with considerable benefit. He wished to allude to the first case where he had used turpentine. Perhaps the want of success was due to the non-employment of castor-oil along with it; but certainly this method of treatment was in this instance a failure.

*Dr G. W. Balfour* had been interested in Dr Jamieson's paper. All of them probably had their pet remedies; but it would be a pity if any one went away with the idea that turpentine was a specific for sciatica, or that sciatica was anything more than a symptom. In the last case he had treated, the remedy he had employed was quinine in large doses, and as this produced sickness, the cure was subsequently completed by full doses (30 minims daily) of the liquor arsenicalis. He had been led to use quinine in this case by the fact of there having been a previous residence in a malarious district in Burmah, while the patient when first seen was pale and chilly, the pain occurring apparently instead of a rigor. One of Mr Syme's favourite prescriptions in sciatica was a dose of croton-oil, and there was every reason to believe that turpentine and castor-oil—which had long ago been recommended by Sir R. Christison as a most efficacious purgative mixture—acted very much in the same manner.

*Dr Jamieson* thanked the members for the kindness with which they had received his maiden effort. His experience of iodide of potash had not been favourable. He did not wish turpentine to be held as a specific, but as useful in those cases of sciatica at the age of tissue degeneration.

VI. *Mr Bell* then read his CASE OF SECTION OF FEMUR FOR DEFORMED UNION AFTER REPEATED ATTEMPTS AT REFRACTURE HAD FAILED.

The *President* thought it was a most interesting paper on the treatment of a troublesome case.

*Mr Chiene* had been much pleased with Mr Bell's description of his case. He was glad he had adopted the true treatment of such cases, viz., that of cutting boldly down on the part with antiseptic precautions, and not Mr Adams's method of sawing subcutaneously. The case seemed to him a perfect one, and he only wished surgeons elsewhere would follow a similar method in similar cases.

VII. *Dr Angus Macdonald* then read an account of THREE CASES OF PNEUMOTHORAX OCCURRING AS AN EARLY SYMPTOM OF PULMONARY DISEASE, which appears at page 712.

The *President* felt sure that the Society was much indebted to Dr Macdonald for his minute and well-put-together account of three interesting cases of pneumothorax.

*Professor Sanders* did not propose at so late an hour to say much, but only to remark that the Society was much indebted to Dr Macdonald for his paper, as a faithful record of cases was always a solid foundation for future practice. As to the relation of pneumothorax to phthisis, while it undoubtedly occurred in the later stages, it was often one of the first symptoms of consumption, using this term in the sense of a tubercular or bronchopneumonic process, where they had some yellow deposit at the free surface of the lung softening and breaking down. The pneumothorax occurred in the early stages because there were no adhesions, while in the later stages it was prevented by these. Some accordingly believed that pneumothorax was almost confined to the early stages, perhaps because it was then more important and striking than when it occurred at an advanced period of the disease. He thought the second case was like an early giving way of a phthisical deposit at the lung surface. The anxiety as to a pneumothorax arose, not from the mere presence of air in the pleural cavity, but from the underlying cause. Of the three cases the first was the most interesting, because although we do meet with permanent cures, yet the recorded cases are rare. It was important, too, from the subsequent consolidation of the apex giving rise to the anxiety that the previous pneumothorax had been due to phthisical softening. From the favourable termination of the case it was perhaps something simpler than a cheesy pneumonia, or it may have been such a lesion running a favourable course. The last case was interesting because rare. In the post-mortem only the chest was examined, but there may have been some cause elsewhere for the lesion which produced the pneumothorax. Some other points deserved remark, if time had permitted, but he wished finally to express the interest with which he had listened to the paper.

*Dr A. Macdonald* thanked the members for the kind reception of his paper. The difficulties Professor Sanders had suggested had just been those which had presented themselves to Dr W. Begbie and himself in his first case. But then taking into account the previous good health, the steady though slow convalescence, and present condition of the patient, he was compelled to think the case had not been one of primary tubercle, but more probably a slight giving way of the lung at a portion of it which was specially weakened, from some unknown cause, probably in consequence of a bronchopneumonic softening. The second case was purely tubercular, as its history showed clear enough to his mind. From a consideration of the family history, the previous hæmoptysis, and the persistent troublesome cough, he fully expected to find tubercle in the third case, but greatly to his disappointment, he might say, almost to his disgust, he had found none. It might have been the case that a more exhaustive post-mortem examination might have revealed some evidence of a septic origin to account for the pulmonary abscess, but though such was very diligently inquired into, we failed to discover anything, so far as we were at liberty to examine the body. Indeed, he could not even get rid of the feeling that the post-mortem examination, instead of clearing up the difficulty, rather increased it, and really supplied no sufficient answer to the question, Why did this man die? He had a sort of self-accusing notion that the original pulmonic lesion was not sufficient to have caused his death, had he been properly managed. The post-mortem examination showed, what he had rather ungenerously been led to doubt, that the physical condition of the patient's right lung was such as to fairly entitle him to be passed for a policy of assurance on his life in March 1876, for the apex of the right lung was found to be perfectly healthy.

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXXVI.—MEETING III.

*Wednesday, 13th December* 1876.—Professor SIMPSON, *President, in the Chair.*

Minutes of previous Meeting having been read and approved,

I. *Professor Simpson* read the following REPORT, drawn up by himself and *Dr Finlay*, of their visit to the International Medical Congress at Philadelphia, as delegates from this Society :—

"In tendering our Report of the International Medical Congress at Philadelphia, our first duty is to thank the Society for the opportunity afforded us, as its delegates, of spending a most enjoyable and profitable week amongst our American confrères.

"The Congress was in every respect a great success. Four hundred and forty-seven delegates, as appears from the printed list, came together, chiefly from America and Canada, but including representatives of the profession from every part of the globe. A Commission had prepared beforehand an elaborate programme of proceedings, which was closely adhered to throughout. Of this we lay a copy before the Society, along with a synopsis of the papers read in the different sections by special request of the Commission.

"The meetings were held within the buildings of the University of Pennsylvania—a handsome structure, having its main entrance flanked by pillars of red granite from Scotland, and presenting in its front wall a series of mural tablets chiefly in the form of an ivy leaf. These, on closer inspection, were found to be commemorative of the classes which have left the University, and at the close of their curriculum built in the slab, and at the same time planted an ivy branch, in token of their clinging regard for their *alma mater*.

"Of the nine sections into which the Congress was broken up and dispersed through the different class-rooms we cannot speak in detail. The proceedings in the Obstetric section were of great interest. The attendance there was always large, and the attention unflagging. Of the four subjects which had been chosen for special discussion by the Commission, two were obstetrical, and two gynæcological.

"The question of the Mechanism of Natural and Artificial Labour in Narrow Pelves, was introduced by Professor Goodell of Philadelphia.

"After defining a narrow pelvis, and describing the more common kinds of pelvic deformity, the following topics regarding the mechanism of labour were introduced for discussion :—

"I. How does the head enter and pass the brim in the flat pelvis? and how in the generally contracted pelvis?

"II. How does the after-coming head behave in the flat pelvis, and how in the generally contracted?

"III. What effect has instrumental interference on the mechanism of labour in such pelves?

"IV. Has turning any mechanical advantage over forceps?

"Regarding the mechanism of labour in narrow pelves, to which the scope of the paper was strictly limited, the following conclusions were reached:—

"1. The unaided first-coming head, and the aided after-coming head, observe in a flat pelvis precisely the same general law of engagement and of descent. Hence version here means art *plus* nature.

"2. The forceps, however, applied in a flat pelvis, antagonizes more or less with the natural mechanism of labour. Hence the forceps here means art *versus* nature.

"3. The aided and unaided first-coming head observe in a uniformly narrow pelvis precisely the same laws of engagement and of descent. But version violates these laws. Hence the forceps here means art *plus* nature: version, art *versus* nature.

"4. At or above the brim of a flat pelvis, the fronto-mastoid, or even the fronto-occipital application of the forceps, interferes less with the moulding of the head, and violates the natural mechanism of labour less than the bi-parietal.

"5 In the flat pelvis, the vectis aids the natural mechanism of labour, and therefore meets the indications better than the forceps.

"The question of the Nature, Causes, and Prevention of Puerperal Fever was introduced by Professor Lusk of New York. He pointed out that puerperal fever was a generic term, including different varieties, which were to be distinguished as (*a*) non-infectious; (*b*) infectious.

"The non-infectious might result from—

"1. Traumatic injuries.

"2. Old peritoneal adhesions.

"3. Disregard of hygienic precaution.

"4. Mental influence.

"The infectious form he regarded as a septic disease, the poison entering the system usually, not necessarily, through local lesions.

"Under this head, the relations of bacteria to puerperal fever, the influence of erysipelas, scarlet fever, diphtheria, etc., upon the puerperal state, and the effect of atmospheric influence, were discussed.

"The causes of the production and spread of the disease were illustrated by a large series of elaborate statistics drawn from the civil records of New York, and from hospital and private practice; and the essay concluded with practical remarks on the puerperal prophylaxis.

"The gynæcological subjects were also ably handled. The question of the Causes and Treatment of Non-Puerperal Hæmorrhages of the womb were reported on, as the Americans express it, by Professor Byford of Chicago, who began by showing that the uterus was prone to hæmorrhage because of the conditions connected with menstruation, and that the causes of metrorrhagia act by aggravating these conditions. These causes he regarded as originating sometimes in the nervous, and sometimes in the vascular system; and of the latter, some operated by increasing the flow of blood through the uterine vessels, and others by retarding the current of blood in them.

"The treatment consists first in removal of the causes; and, secondly, in the use of surgical, mechanical, and medicinal means to check the flow.

"The question of the Treatment of Fibroid Tumours of the Uterus was introduced by Dr Washington L. Atlee, who gave the results of his own large and long experience. He divided fibroid tumours into two groups, according as they were or were not accompanied by hæmorrhage. The fibroids usually accompanied with hæmorrhage might be found—

"*a.* Occupying the vaginal canal, and were to be removed by torsion or the ecraseur.

"*b.* In the cavity of the uterus, whence after dilatation of the cervix they were to be removed by the bistoury.

"*c.* Submucous and interstitial, when they were best treated by administration of ergot, but sometimes required to be attacked with the knife.

"*d.* Properly interstitial, when the best effects were to be expected from ergot and sorbefacients. Under this head also he grouped recurrent fibroids.

"The non-hæmorrhagic fibroids might be (*a*) interstitial, but subperitoneal, or (*b*) sessile peritoneal, or (*c*) pedunculated, or (*d*) interstitial cervical, or (*e*) myomatous or fibro-cystic. Among these were found the cases where, from large size and continuous growth of the tumour, its removal by the abdominal section was warranted and required. Ergot had no influence in modifying such cases, but he believed in the absorbent influences of muriate of ammonia.

"We do not attempt to sketch the other papers that were read before the section, or the debates, able and animated as they were, which followed. We must, however, express our admiration of the eloquent Address in Obstetrics, delivered by Professor Parvin of Indiana, in which, in opposition to an opinion of Dr Tyler Smith, he claimed for American midwifery affiliation with the midwifery of Britain rather than with that of France, and held up ovariotomy and the operation for vesico-vaginal fistula as her two noblest offerings to medicine and to humanity.

"It was a cause of much regret to us that we were obliged to leave Philadelphia on Friday afternoon, and so were prevented attending the closing meetings of the Congress. We gladly seize, therefore, this opportunity of stating how heartily we endorse the sentiments of admiration, esteem, and gratitude expressed in the address of congratulation which was presented in the name of the British delegates by Dr Hare of London, and which runs thus:—'The delegates from Great Britain to the International Medical Congress of Philadelphia beg to congratulate the President and the several

Committees on the complete success of the Congress, on the high value of the various addresses presented to it, and on the forward impulse which it has given to the progress of medicine in the widest sense of that word. They desire at the same time to express in the strongest and warmest terms their sense of, and their thanks for, the unmeasured kindness and courtesy, and the unbounded hospitality with which they have been received on this Centennial occasion, and to add, that they will carry back with them a most grateful recollection of that warm right hand of fellowship which has been so warmly extended to them by their brethren of the United States.' (Signed) ALEX. R. SIMPSON.
" WM. FINLAY."

*Professor Simpson*, having read the Report, explained that he had found it impossible to devote a sufficient length of time to this very pleasant holiday, and *Americanis Americanior* had spent less than a week in seeing the New World. He had just been able to see enough to tempt him to form a resolution to spend some longer vacation there; and in the meantime he deemed himself happy in having had the opportunity of making the personal acquaintance of many men whom he had only known before by reputation. Besides, not to speak of the great Centennial Exhibition, etc., etc., he had seen a sight which was of itself reward enough for having crossed the Atlantic. That was a collection of what Hodge used familiarly to call his "abortions." All Fellows of this Society had at times, and some of them many times, made use of Hodge's pessary. But for himself, it was not till he saw that collection in Philadelphia that he realized how much thought and time her great obstetrician had expended ere he discovered the apparently so simple and so widely useful lever pessary. For a long time the material of which Hodge had his pessaries made was silver-gilt; and at one time during the American war, when the precious metals were scarce, the greater number of the obsolete forms were sent to the mint and turned into coin. A sufficient number had been retained, however, to show the many various phases of evolution through which the pessary had passed; and often, when some supposed new and improved modification of his instrument would be sent to him, with a different curve, or crook, or crutch adapted to it, he would pass it to his son with the quiet remark, "Here's another of our old abortions."

*Dr Finlay* corroborated what the President had said, and added, that the expedition to America was not at all such a formidable undertaking as might be supposed, and that he could ensure for any of the Fellows who might visit it a very cordial reception.

*Dr Matthews Duncan* proposed that the thanks of the Society be accorded to the delegates who had gone to represent them in the International Medical Congress. The motion was seconded by *Dr Keiller*, and agreed to by the Society.

II. *Dr Underhill* showed for *Dr Macdonald* the PLACENTÆ from a case of triplets. It will be noticed that one of the placentæ is complete in itself, and provided, so far as a hasty inspection can tell us, with its membranes complete. The other two have a common chorion and amnion, with the placentæ intimately united, but yet distinctly two, as indicated by a line which may be seen both on the fœtal and maternal aspects. It might safely be assumed that in the latter case the children were the product of a single ovum, and agreeably to that assumption, according to Hyrtyl's law, the two children in their common sac were similar in size and appearance in every way, only that one of them had been dead a day or two. The united weights of the children amounted to about 14 lbs., and as there were at least 3 pints of liquor amnii along with the latter two children, and 1 with the first, if we assume the placentæ to weigh, with their blood, 2 lbs., it follows that the ovum must have weighed at least 20 lbs. The patient was greatly distended, and had much œdema of lower limbs, but no albuminuria. The children all presented by the head. The first stage was very slow and painful, and, to save the strength of the patient, I removed the first child's head out of the outlet of the pelvis by forceps. I knew then I had to deal with twins at least. The patient had no post-partum hæmorrhage; pulse after delivery was 72. In the afternoon—she was delivered at 11.30 A.M.—an attack of headache with blindness came on, and an eclampsic seizure followed at 9 P.M. Her urine now was albuminous, but cleared in 24 hours afterwards. She is now doing well.

III. *Professor Simpson* exhibited a preparation from the Maternity Hospital, illustrating the death of the child with excessive TORSION OF THE UMBILICAL CORD. The notes of the labour were taken by Mr H. S. Nelson, House-Surgeon. "J. F., age 20, admitted 8th December 1876, pregnant for the second time. Her first child was born when she was 15; it was alive, and still lives. Her recovery was protracted, and for about a year she suffered from severe back-ache. Her menstrual periods occurred within the four weeks, tended to excess at times, and during the intervals she occasionally suffered from leucorrhœa. No history of any specific affection can be elicited. Patient states that her last menstruation commenced on the 10th of March, and ceased on the 14th. She does not recollect when she quickened. For a fortnight previous to admission she had not felt any movements. The fœtal heart could not be detected on auscultation. At 8 A.M., 8th December, patient awoke and found a bloody discharge coming from the vagina, and a clot was expelled, but she felt no pain. She was sent to the Royal Maternity Hospital, and examined at 10.35, when the discharge of blood was found to be still going on. Vaginal examination showed the bag of membranes in the vagina, and visible on separating the labia. Os was fully dilated, and the hand presented. At seven minutes to eleven ergotin (10 mm. Professor Simpson's formula) was administered hypodermically. The membranes ruptured at 11.10, and the right foot protruded. At 11.30 the child was expelled, and without dividing the cord the placenta was expressed. A considerable gush of liquor amnii followed the expulsion of the fœtus." The child, a female, with a harelip and split palate, appeared about seven months old, and with the placenta weighed 3½ lbs. Desquamation of the cuticle had taken place over nearly the entire surface of the body, and the bones of the skull were becoming lax; so that it must have been dead for three weeks. It measured 13½ inches in length. The umbilical cord, which measured 18½ inches in length, was of average thickness in nearly all its extent. But the torsion of it was excessive, so that close to the body of the fœtus it is as narrow as a crow-quill; and again at the distance of a foot from the umbilicus, the torsion is associated with a narrowing of the cord. In the concavity of the helices the connective tissue is unusually dense. The relation of the tear in the membranes to the placenta shows the low implantation of that organ; and it is worth notice that the cord runs into the placenta close to its margin, but at the point in its circumference that had lain highest in the uterine cavity. One of the lobules was atrophied.

IV. *Professor Simpson* also showed a preparation illustrative of INTRA-UTERINE PERITONITIS IN THE FŒTUS. The patient who had borne the child had the following history, as drawn up by Dr Mackay, House-Surgeon to the Maternity Hospital:—"A. B., 25 years of age, second pregnancy, was, after a normal labour, delivered on 4th December (4.45 A.M.) of a female child weighing 6 lbs., which only lived an hour. The patient, a pale emaciated woman, was found to have several discrete condylomata on the right labium, one on the left nympha, and a cluster of large ones on the perineum and right verge of the anus. The inguinal, especially on the right side, and nuchal glands, were enlarged and indurated. She had a male child on 10th February 1874, which died when 1 year and 9 months of age—said to have been born with hemiphlegia, and died in convulsions. Patient has been mostly a factory hand, but more lately a domestic.

"*Present Pregnancy.*—She last menstruated in the beginning of April, and about the same time, as she avers, had sexual connexion for the last time, and with the father of the present child. Since then she has been from place to place until she left a Magdalene asylum for the Poorhouse at the end of August. Not until a fortnight after her admission there did she feel anything the matter with her. She then complained of pain on micturition, and discovered, so she says, several elevated 'blister like' spots on the labia, and a swelling within the right one, which gradually increased to the size of a small egg, suppurated, burst, and healed along with the spots; no trace, however, can be found of any such abscess. About the same time appeared the condylomata on the perineum, which have persisted ever since. Her throat and palate also about that period were sore and inflamed, so that she had difficulty in speaking and swallowing; the particular character, however, of the throat affection cannot be clearly ascertained. She does not remember having felt the inguinal glands enlarge, though she felt some uneasiness there about a month ago, when also the condylomata reappeared on the labia which are now there. About two months ago, the inside of her ears 'broke out into little sores, and ran matter,' and several papular spots, the size of a sixpence, appeared on the forehead. These lasted three weeks or a month, and have left no trace. She denies having had any other skin eruption, and her hair never came out. Apart from the history of the syphilitic attack, patient states that ever since the beginning of September she has had more or less abdominal pain, accompanied by constipation and some tympanitis; and Dr Williamson, Leith, who had her under his care in the Poorhouse, says that he suspected and treated her for some degree of deep-seated peritonitis. These symptoms have now disappeared. With the exception of the local affections, she has now no indications of disease, and has been making a satisfactory recovery from her labour."

The fœtus presented an enlargement of the abdomen, the cavity of which was found on section to be distended with fluid of a reddish tint. There were traces of inflammatory action over the whole peritoneal surface—flakes of lymph lying on the surfaces of the liver, spleen, and several of the other viscera. It was interesting to note that bands of adhesive matter passed from the fundus uteri to the abdominal walls and the lower intestinal coils, which might easily have led to permanent displacement if the infant had survived.

V. *Dr James Young* communicated particulars of a case of EARLY VIABILITY, by Dr Porteous of Pathhead. The child when born was 6 months 8 days, or 189 days, old, 11 inches long, weight not ascertained; nails and hair well developed, and lived for 35 hours. Dr Young likewise mentioned a case which Dr Porteous had communicated, that of a girl in whom menstruation had begun at the 7th year. The girl's mother, it was stated, had begun to menstruate at 11 years.

*Dr Milne* remarked that he was at a loss to know how the case could be considered one of early viability, as the child died in 35 hours.

*Dr Wilson* agreed with Dr Milne in considering that there was nothing unusually peculiar about the case. He had met with many instances in which children, quite as premature as this one, had lived for many hours.

*Dr Bruce* had a case in which a six-months child lived forty hours, and such cases were not uncommon.

*Dr Keiller* would prefer to call the case one of live birth. He had seen similar cases live as long. He recollected a fœtus of four months that lived a short time, but which, of course, was not "viable," although it was a case of "live birth," inasmuch as the fœtus presented certain indications of life after its expulsion. The case of Dr Porteous, although an interesting enough one, was not, properly speaking, "a case of early viability," which term can only be applied to a living child sufficiently developed to be capable of continuing to live *after its birth*. The term *viable* is often confounded with that of *live birth*, which does not necessarily include a child's *viability*, or power of not only surviving its birth for a limited time, but of continuing its existence under ordinary or extraordinary care. The questions of "live birth" and "viability" were important, especially in a medico-legal point of view.

*Dr Craig* also objected to the case being termed one of early viability.

VI. *Dr Matthews Duncan* then read a paper on THE LACERATION OF THE EXTERNAL GENITAL ORGANS (EXCEPT THE HYMEN) DURING LABOUR IN PRIMIPARÆ.

*Dr Keiller* thought the paper most interesting, and some points in it worthy of remark. Owing to the late hour of the evening, he would not make any remarks, but proposed the adjournment of the discussion till next meeting. This was seconded by *Dr Milne*, and agreed to by the Society.

# Part Fourth.

## PERISCOPE.

A CERTAIN SIGN OF REAL DEATH FOR PHYSICIANS AND NON-PROFESSIONAL MEN.—Dr Hugo Magnus of Breslau, assistant to Professor Forster, thinks he has solved the problem of the Ourches prize by a very simple experiment, which any one can make. The war of 1870 and '71 prevented him presenting his work to the Academy of Paris. After an historical glance on study of death, on the signs of death in its general and particular aspects, the author grapples the question, basing it on physiological laws. In the human organism there are certain symptoms whose function, though reduced to a minimum, are indispensable to life, and the suspension of which inevitably causes death. These are the vascular and respiratory systems. As long as life exists, their functions are never completely arrested, their vitality being combined in an intimate manner to the uninterrupted circulation and to the continued supply of oxygen. Their action may momentarily be suspended without causing death, but this secession of action is so short that it need not be taken into consideration. It is then on the organs of these groups that the author has directed these experiments; and this is how he proves the existence or non-existence of the circulation, and consequently life. "If with a cord," he says, "we completely ligature a member, and by choice a finger, the points situated below the ligature will quickly assume a colour which, at first red, becomes more and more deepened, until it is of a reddish-blue. Further, all the parts situated below the ligature, from the extremity to the cord, will have an uniform colour; and even round the ligature a circle will be observed which will not be reddish-blue but white." This experiment, which on the living always produces a reddish-blue colour, always gives on the dead body a negative result, whatever be the cause of death. On many corpses, as also in certain severe maladies (particularly of the blood), a bluish coloration of the nails and of the ends of the fingers may be observed. This colour has no influence on the experiment precognosced by the author. If a finger be tied, as long as life remains in the organization, all the parts situated below the ligature will assume a uniform reddish-blue colour; this colour not presenting itself, or but having a circumscribed area, one may be sure that life is extinct. From experiment the author passes to the physiological explanation of the phenomena produced (a mechanical obstacle to the return of the venous blood in the capillaries and in the veins produces a reddish-blue colour). The white circle round the ligature is produced by a partial arterial anæmia; the arteries being compressed, there is an obstacle to the afflux of the blood, consequently the arterial anæmia of the part of the member situated below the ligature occurs, anæmia hidden by the venous hyperæmia. But round the ligature the arteries and veins being equally compressed, and consequently empty of blood, the arterial anæmia not being at this place hidden by the venous hyperæmia, it is visible. By the colour of the ligatured member one may then prove that the circulation has place in this member, and that the ligature obstructs it. The circulation, though, perhaps, as feeble as possible, the ligature will always be an obstacle in preventing the return of the venous blood, which will give to the member a colour more or less intense according to its quantity; the discoloration will be always reddish-blue by virtue of the properties of venous blood, at least provided the skin be not too thick to allow of its appearance. In this last case we make the experiment on a member of a more delicate skin. The member on which we experiment should not be too large, in order that its compression may be complete, as in a finger; in the pollices, where the skin can be strongly compressed against the bone, the arrestation of the circulation is sufficiently perfect. If the experiment cannot be made on the digits or pollices, it is admissible to perform it on the auricle of the ear.—*Revue de Thérapeutique Medico-Chirurgicale.*

PANCREATOMY.—In the "American Medical Weekly" of 11th November 1876, there is published the report of a case of pancreatomy by Dr B. B. Allen, of Sebastopol, California. The patient was an Indian, thirty-five years of age, who had been stabbed in many places. From a wound just above the left kidney the pancreas protruded. The protruded mass was seven inches in length. It had been exposed for about twelve hours, and was "partially putrefied." He received this wound on the 9th of October. On the 11th, after chloroforming the patient, the protruded mass was surrounded by ligature, and the distal end (all beyond the ligature) immediately exsected. The patient had no unpleasant symptoms whatever, and has made a complete recovery. As the exchanges seem to regard this as a very remarkable case and a most remarkable recovery, it will not be out of place to offer a few facts in this connexion. In the "Transylvania Journal of Medicine," vol. i. p. 116, there is published a case which was treated by Drs Caldwell, Roberts, and Heard. The patient was a negro, owned at the time by Judge Bibb. The part protruded was four inches in length, and was gangrenous. It was removed by the bistoury. The patient recovered. Kleburg, of Odessa, reports the case of a soldier who was stabbed, with a portion of the pancreas protruding; the mass was ligated and excised, and the patient recovered. Laborderie relates the case of a young girl who was wounded by falling upon a knife. The pancreas protruded through the wound (in the epigastrium), and was ligated and excised. The girl made a complete recovery.—*Gazette des Hôpitaux*, 1856. Surgeon J. G. Thompson, of the Seventy-Seventh New York Regiment, reports the case of a soldier who was wounded at Cedar Creek, 19th October 1864. The pancreas protruded so as to present a mass the size of a hen's egg. A silver wire was passed around the mass, and it was successfully removed. The patient recovered completely. Many fatal cases of protrusion of the pancreas are on record, and also a large number of fatal cases of wounding of the pancreas where the organ did not protrude. These five cases of protruded pancreas, with operation and recovery, show that the general impression in regard to the fatality of such wounds is not entirely sustained by the facts. Surgeon George A. Otis, of Washington, D.C., has given much time to the examination of such wounds, and his report to the Surgeon-General, U.S.A., is a model of accurate, fruitful research; and, like all of his papers, a model also of clearness and brevity. The writer is indebted to him for many facts in regard to such wounds, and for many of the facts herein presented. The case reported by Dr Allen to the "American Medical Weekly" is being closely observed, and a further report of it will be given.

LISTER'S ANTISEPTIC METHOD IN OVARIOTOMY.—Professor Lister's late visit to this country seems to have given a new impulse to antiseptic surgery Van Buren has adopted it with success, and is lecturing on it to his class at Bellevue with great enthusiasm. Stephen Smith has also adopted it with the same success, and is teaching it most earnestly to his class at the University, and other surgeons must take it up. I have often wondered why it had not been used in ovariotomy. Lister told me it had not been done in Great Britain. He advocated it strongly, but Spencer Wells and Keith have had such wonderful success in their operations that they did not feel justified in trying any new method. I would have used it long ago if I could have found a convenient and ample spray-producer. A fortnight ago I heard that Dr Sass had perfected an apparatus, and had tested it in operations by Van Buren, Stephen Smith, and others. I saw Dr Sass, and he kindly consented to bring his apparatus, and apply the carbolic spray for me in a case of ovariotomy. The patient, forty-seven years of age, noticed a tumor the size of an orange in the right iliac region last April. She consulted her family physician, who pronounced it an ovarian tumor. In June she consulted Dr Thomas, who wisely told her the time had not arrived for an operation. On the 20th July she went to Philadelphia to see Dr Atlee, who gave her the same good advice. I saw her on the 20th September. I have never seen any one so anxious for an operation. I dissuaded her from it, advising her to return home, and wait at least till next spring. I told her the tumor did not weigh more than ten pounds, and that an operation was not justifiable till she vomited her food and began to emaciate. I saw her a month later. She declared she had not the strength to make the journey home, and that she threw up every time she took food. I still refused to operate. She wrote to Dr Atlee, and he replied on the 6th November, "I can scarcely think a tumor so small can affect your general health so seriously. But if your emaciation and debility are the result of the presence of the tumor, then by all means it should be removed." I believe her vomiting and consequent emaciation were mainly the result of mental and moral causes. Whatever the cause, her declining strength and a recent fugitive attack of peritonitis warned me not to procrastinate the operation any longer. The operation was done on Thursday, the 23d November last. I am particular in fixing the date, because I believe it inaugurates a new departure in ovariotomy. Dr Sass directed the spray, which covered the seat of operation with a delicate carbolic mist. The hands, sponges, and instruments were all dipped in carbolic water. The operation and dressing lasted forty minutes, the spray being kept up all the time. It could have been continued two hours, if necessary. There were no adhesions. The peritoneal cavity contained six or eight ounces of a reddish serum. The peritoneal membrane was everywhere deeply congested. This fact explains the presence of reddish serum and the previous attack of peritonitis. The pedicle was very short, and at least three inches broad. It was tied in three sections with strong twine, and drawn out and fixed in the lower angle of the wound, clamp fashion. The external incision was closed by sutures, and a carbolized dressing applied. The pulse never rose above 90, nor the temperature over 101°. Convalescence was fully assured in forty-eight hours, and the patient is now quite well. The tumor was polycystic on right side, and weighed fifteen pounds. I hastened to lay this case before the profession merely to urge the adoption of Lister's antiseptic method in ovariotomy, which, I am sure, will prove as valuable in this operation as it has in general surgery. Dr Sass's apparatus answered its purpose admirably, and I think he has rendered us a great service in bringing it before the profession at this time.—J. MARION SIMS, M.D., *in New York Medical Record*, 9th Dec. 1876.

GASTRIC ULCERS AND PERFORATING ULCER OF DUODENUM AFTER A BURN. By Mr STOKES.—This specimen was taken from a patient aged eighteen years, who was admitted into the Richmond Hospital under my care on the 28th of last month suffering from the effects of a very severe burn. His friends informed my clinical clerk, Mr Ryan, that he was subject to epileptic fits, and that in one of them he fell forward into the fire and sustained the injury for which he came to the hospital for treatment. His face was very extensively burned, and also his neck, and almost the entire front of the thorax, the upper portion of the abdomen, and also the right arm, the degree being the fourth of Dupuytren or the third of Hebra. When admitted into the hospital he was suffering from great collapse, from which, however, he rallied. He went on from that until the day previous to his death extremely well. There was no evidence during the entire time of his stay in the hospital that there was any internal mischief at all. He took his food well and was cheerful, and, on the whole, we formed a not altogether unfavourable opinion as to the chance of his ultimately getting over the effects of the injury, with, however, of course, the result of great deformity should he live. However, on the seventeenth day after his admission he had a severe attack of hæmatemesis, but there was no evidence of peritonitis whatever. Shortly after that he became collapsed, and died on the morning of the eighteenth day. On examining him we found very extensive ulceration, involving not merely the duodenum, but also the stomach. There was a very large oval and clearly-defined gastric ulcer at a considerable distance from the pylorus; at the junction of the stomach and the duodenum there were two small ulcers; and farther on a large oval perforating ulcer in the duodenum. There were also two smaller duodenal ulcers, which, however, were not perforations. Only one specimen similar to this has been exhibited to the Society, and it was by the late Professor Smith, in 1858; the "Proceedings of the London Pathological Society" also only show two examples of a similar kind. The perforation, therefore, is one of considerable rarity, and makes this case one of great interest. Another feature of interest is the existence of gastric ulcers, which

I have never seen before in cases of this sort. Examples of it may occur in the literature of the subject, but I am not aware of them. Another peculiarity of the case was the absence of all peritonitis. This was also a characteristic of the case exhibited by Professor Smith. In his case three ulcers perforated all the tunics of the intestine. In this case no blood was found in the intestine, and there was no evidence of peritoneal inflammation. The three things rendering the case one worthy of exhibition were—first, the existence of gastric ulcers; secondly, the large perforating ulcer in the duodenum; and, thirdly, the absence of all peritoneal inflammation. The patient became collapsed immediately after the vomiting of blood on the evening of the seventeenth day, and, as I have mentioned, died on the morning of the eighteenth day, 25th March 1876.—*Dublin Journal of Medical Science*, October.

Dr Hutchison, Providence, in the *Boston Medical and Surgical Journal*, relates some experiments he has made to demonstrate the differing surgical effects of "quantity and tension galvanic currents." They are very elementary, and show simply how the one can be used only as a galvanic cautery, the other for electrolysis. In the same number (for June) Dr Minot discusses Lesser's interesting experiments on what he calls auto-transfusion. By encircling the limbs by Esmarch's bands, and in other ways driving blood to the heart, vitality may be restored in animals after excessive hæmorrhage.

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## MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris, and of the Pharmaceutical Society of St Petersburgh; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

Bromide of Sodium.—Dr W. A. Hollis records eleven cases of epilepsy in which most favourable results were obtained with bromide of sodium. He gives from 15 to 40 grains of the salt in an ounce of camphor water, or other vehicle, thrice daily. For a child two years old, he advises the administration of three grains in two drachms of decoct. cinch. pallid. three times a day. Dr Hollis observes that the salt appears to exert a greater influence on the so-called "convulsions" of children than it does on the more confirmed epilepsy of adult age.—*British Medical Journal*, 1st July.

Uses of Large Non-Emetic Doses of Ipecacuanha.—In two pamphlets on this subject, Dr Alfred A. Woodhull records his experience of this treatment in dysentery and intermittent fever. In dysentery, he first quietens the stomach, which should be empty, with opium, and then gives a large dose of ipecacuanha (25 or 30 grains). The recumbent posture and abstinence from food or drink for at least four hours should be strictly enjoined. The dose may be repeated in from two to six hours. He thinks that when the stomach is empty the medicine acts non-emetically in direct proportion to the severity of the attack. Of twenty-three cases of intermittent fever treated by ipecacuanha in doses of from 1 to 20 grains, all were cured but one. Dr Woodhull thinks that one or two grains, given every three or four hours, is the best method for ordinary chills. Large doses did not appear to exercise a beneficial effect in proportion to their size. If large doses be used, it is well to guard them with small quantities of opium, and to observe the precautions of recumbent rest and abstinence.

APOMORPHIA.—Jurasz of Heidelberg finds that solutions of apomorphia will keep for several months; they gradually become dark green, but do not lose their emetic power. Subcutaneous injection produces no ill effects beyond slight local redness. Jurasz prefers the skin of the back as the site for injection; he uses a one per cent. solution. It should be very cautiously employed in the case of children, too large doses being apt to produce violent vomiting and symptoms of collapse. For children ten years of age, he advises a dose of one-tenth of a grain. As a rule, vomiting comes on in about ten minutes, and all disagreeable symptoms disappear in one or two hours. After the vomiting there is a disposition to sleep, and symptoms of depression. Salivation and sweating continue for some little time. It has been found useful in croup, bronchitis, and capillary bronchiolitis in children; and in emphysema, with severe bronchitis and dyspnœa in adults.—*Deutschs Archiv für Klin. Med.*, Bd.xvi. i.

ARNICA.—Dr A. Patze, writing in *New Remedies*, June 1876, respecting the use of tincture of arnica externally, states that, according to Schuchardt, experiments with arnica have yielded the following results:—Small doses accelerate the pulse, raise the temperature of the skin, increase the secretion of urine, and cause tremor of muscles. The violence of these symptoms increases with augmentation of the dose. Injections of an infusion of the flowers into the veins caused much excitement, followed by intense languidness, vertigo, and even death: on examination, the organs of the chest and abdomen, and the cerebrum and spine, were found engorged with blood. In man, any part of the arnica plant applied to the skin causes an itching, burning sensation, accompanied by redness. Small doses of from 4 to 10 grains exert an irritating effect on the fauces and larynx, and on the stomach and alimentary canal, causing a burning, scratching sensation, cardialgia, abdominal pains, nausea, vomiting, and frequent evacuations; the circulation is quickened, and the bodily warmth is increased; the secretions also are increased, especially those of the kidneys, skin, and lungs. Continued use of it will cause numbness of the head, vertigo, mental depression, restless sleep, oppression of the lungs, jerking pains, etc. The hot infusion acts more surely than the tincture, and the flowers are more exciting than the root. Arnica may be indicated in diseases which manifest a character of torpor, or whenever an acceleration of the circulation is desirable. In Germany it is extensively used, especially in cases where the vitality of the nerve-centres, brain and spine, is oppressed; in exhaustion, paralysis consequent on apopletic strokes, rheumatism, catarrh, pleurisy, pneumonia, in traumatic commotions of the brain, in typhoid fevers with torpor in paralytic affections, etc. The external use of it is very limited, and it is especially contraindicated in recent traumatic cases. It should never be applied before all tendency to inflammation is removed. About seven days after the injury, Dr Patze advises its application in the following form:—

℞. Tinct. flor. arnicæ, ℥ss.
Aceti ℥ss.
Aq. camph. ℥vi.
M. Sig. "For external use."

COLCHICUM.—According to J. Rossbach, colchicum produces complete loss of sensibility from paralysis of the peripheric and centric nerve-endings. Reflex excitability is consequently abolished. On the other hand, the motor nerves and the muscles retain their excitability. In many animals the paralysis is preceded by a stage of excitation. The circulation is only slightly affected, and the heart continues to pulsate even after paralysis of the central nervous system has commenced. The blood-pressure long remains unchanged, and paralysis of the inhibitory nerves of the heart occurs at a late period. The respirations gradually become less and less, until they are completely arrested; hence it is concluded that there is gradual paralysis of the respiratory centre. In warm-blooded animals, the whole alimentary mucous membrane is swollen and strongly injected, and the intestine contains bloody mucus; there is diarrhœa, vomiting, and colicky pains; the kidneys are strongly hyperæmic, and their secretion is diminished. The action of colchicum is very slow, and it is remarkable that the amount of the dose has scarcely any perceptible influence on the intensity or rapidity of its action; doses smaller than are sufficient to cause death have scarcely any action.—*Pflüger's Archiv*, B. 22.

HYDROBROMATE OF CICUTINE.—Dujardin Beaumetz considers that cicutine and its salts affect the nervous system in the same manner as curare, but the pneumogastric nerve is affected by the former drug. Cicutine salts may be given in convulsive affections, and particularly for reflex symptoms connected with the pneumogastric nerve. Thus, they may be of service in convulsive cough, asthma, hooping-cough, in certain forms of hiccup in dysphagia, vomiting, etc. Also in the convulsions of tetanus, in infantile convulsions, in chorea, and in neuralgia. Cicutine is best given hypodermically; when thus administrated, hemlock appears to act very differently to what it does when given by the stomach, the intestinal juices apparently destroying, in a certain degree, the properties of the drug. A very small quantity used hypodermically will produce a greater effect than a considerable dose taken by the mouth.—*Bull. Gen. de Thér.*, 15th July 1876.

M. Mourrut of Paris has succeeded in obtaining crystals of hydrobromate of cicutine or conia by directly combining the hydrobromic acid with the alkaloid. The salt crystallizes in colourless prismatic needles, and, unlike the other salts of cicutine, is stable and not deliquescent. The crystals are very soluble in water and alcohol, are inodorous and almost tasteless. Its effects on the lower animals were to produce paralysis and sleep. During the greatest depression, the respirations and beats of the heart were distinctly perceived, and there was no serious impairment of sensation. From the observations of Dr Landur, it would appear that the salt is likely to prove useful in hooping-cough, asthma, cough of phthisis, and the pains of dentition. He gave it in doses of two milligrammes for children one year old, and one centigramme for adults. Dr Regnault administered it hypodermically in a case of sciatica; after two injections the pain ceased. He also gave it with success in a dose of three milligrammes to a phthisical patient suffering from neuralgia.—*Bull. Gen. de Thér.*, 30th May 1876.

DATURIA.—The following are the conclusions of Jobert relative to the mydriatic action of daturia. It is three times as active as atropia, and hence its dose should be one-third less. When introduced between the lids it does not cause pain or confusion of vision which attend the similar use of belladonna. Its effects are more constant, and its action is more persistent.—*New Remedies*, June 1876.

DIGITALIS.—Last year MM. Feltz and Ritter communicated to the French Academy of Sciences the results of their investigations as regards the action of digitalis and of the biliary salts. The biliary salts, and infusion of digitalis in non-poisonous doses, lower the temperature about 1° C. The arterial tension is lessened by both, but more so by the digitalis. They both render the respirations slightly irregular. The pulse is lowered by both, but in the case of digitalis the descent lasts a very short time, and is followed by acceleration; in the case of the biliary salts the descent lasts a much longer time, and is not followed by an abnormal elevation. Animals that are put under the influence of the biliary salts lose less of their weight than those that are digitalized. In poisoning animals with the biliary salts and with digitalis, in cases in which the pneumogastric and sympathetic nerves have been divided, the pulse is affected in the first case, and not in the second, while the temperature and respiration remain the same. The authors conclude from this, and from other considerations, that the effect of digitalis is exerted more on the nervous system than upon the blood and muscular tissue, and that the reverse is the case with the biliary salts. In animals poisoned with digitalis, they always found the heart in a state of relaxation, and never in that condition of tetanic rigidity characteristic of poisoning by the biliary salts. These two poisons have hitherto been supposed to be almost identical in action.

LUPINE.—Seeds of *Lupinus sativus*, or white lupine, are employed as an emmenagogue vermifuge, and externally in resolvent poultices. According to R. Bellini, the seeds contain an active principle, soluble in water; this is toxic, both to man and the lower animals, but is not directly irritant to surfaces to which it is applied. When it passes into the circulation, it exerts an enfeebling action on the brain, the vaso-motor, and the sensory nerves of the voluntary muscles, and on the muscular fibres, the peripheral expansions of the sensory nerves being the parts most affected. This depressing action lasts only while the poison is in the organism, and is counteracted by stimulants. Decoction of lupine should not be used as a toxic or vermifuge, but as a parasiticide it may be employed externally without danger.—*New Remedies*, June 1876.

# Part Fifth.

## MEDICAL NEWS.

FOREIGN HEALTH STATISTICS.—United Kingdom of Great Britain, during four weeks ending 21st Oct., 22,851 births and 12,100 deaths were registered in London and twenty other large towns, and the natural increase of the population was 10,751. The mortality from all causes was, per 1000:—In London, 18·25; EDINBURGH, **15·50**[1]; Glasgow, 20·75; Dublin, 20·25; Portsmouth, 19·25; Norwich, 22·25; Wolverhampton, 21·25; Sunderland, 17·50; Sheffield, 21·75; Birmingham, 19·75; Bristol, 19·75; Liverpool, 22·25; Salford, 27·50; Oldham, 26·75; Bradford, 18; Leeds, 20·75; Hull, 18·50; Newcastle-upon-Tyne, 16·25; Leicester, 22; Manchester, 24; Nottingham, 17·50. Other foreign cities at most recent dates, per 1000:—Paris, 22; Rome, 23; Vienna, 23; Brussels, 19; Berlin, 22; Hamburg, 23; Calcutta, 27; Bombay, 24; Madras, 42; Amsterdam, 19; Rotterdam, 23; The Hague, 26; Christiana, 20; Breslau, 22; Buda-Pesth, 27; Turin, 23; Alexandria, 59; Copenhagen, 21; Munich, 30; Naples, 22.—*New York Sanitarian*, Dec. 1876.

[1] The Capitals are our own.—ED. *E.M.J.*

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*Five Cases of Complete Inversion of the Uterus; with Remarks on the Operation for Chronic Inversion.* By J. MATTHEWS DUNCAN, M.D.

SOME cases of partial uterine inversion have come under my observation, but the following five are all the cases of complete inversion which I have seen. Two of them are cases of acute inversion post-partum. Of these, one was fatal, while the other proved of only temporary and trivial importance. The remaining three are cases of chronic inversion post-partum. They occurred in my practice in the Royal Infirmary. Of the three, two were cured—one by forcible taxis, and one by operation combined with taxis—while the third died after the operation of removal of the organ.

CASE I.—Mrs M., a fine healthy young woman, had been frequently under my care for various passing ailments. She had gone through a first pregnancy and subsequent confinement without any serious alarm being excited. She was residing in Perthshire during her second pregnancy, and enjoyed good health till she reached nearly the end of the eighth calendar month, when she was unexpectedly seized, on a Sunday evening, with violent and uncontrollable sickness and vomiting, which continued, with only brief intermissions, till her death on the following Saturday. On Friday I was summoned to visit her in consultation with Dr Gordon. On the night preceding my visit she was described as having been most alarmingly ill, incessantly vomiting; pulse 130. On the evening of Friday, when I saw her, she was cheerful; her pulse 90; vomiting had ceased during the day; had been able to retain nutritious fluids; but she was very weak and exhausted, and in great dread of the returning sickness and vomiting. The pain of the latter efforts she described as very great, and as if her hips were being torn off. Ordinary antiemetic treatment had been diligently pursued, and was continued. The bowels, which were habitually constipated, had just been freely moved by turpentine, and many scybala discharged. It was resolved to induce labour, and I inserted into the cervix uteri a moderate-sized sponge-tent. She passed the night without great disturbance by vomiting; and as labour showed no signs, I departed for Edinburgh by an early train. When Dr Gordon paid his visit, he withdrew the sponge-tent, and finding no indications of active labour, he ruptured the membranes. Labour now began and progressed rapidly, the child being born alive about 10 A.M. The placenta was spontaneously expelled almost immediately after the child, and the uterus became well contracted. She had much sickness during the labour, and delivery did not put a stop to it. About five minutes after delivery, flooding commenced, while the uterus was in an otherwise ordinary condition. But soon it became inverted; the flooding continued profusely, and she died about half an hour after the birth of the child.

When the alarming symptoms began after delivery, I was again telegraphed for, but, of course, did not arrive till some hours after death. Before I left the house, a post-mortem examination of the abdomen was made, by the considerate permission of the husband.

The uterus was within the vagina. When grasped by the hand, it felt as if of about the size of a small fœtal head. There was no, or very little, constriction at the base of the tumour formed by the internal os uteri. The cervix was elongated, thin, soft, and relaxed, not inverted, encircling the upper part or base of the uterine tumour. On laying open the abdomen, the organs were observed to be exsanguine, but otherwise healthy. Near the brim of the pelvis could be seen the opening of the peritoneal cavity of the inverted uterus, with the ovaries, tubes, and round ligaments arranged in it and upon it. The peritoneum was entire and apparently uninjured. In the right ovary was a thin-walled cyst of the size of a walnut.

I now made some experiments on the inversion. It was easy to pull down the uterus so as to make it procident. This produced inversion of the cervix. It was easy to replace the uterus in the vagina, and this produced replacement of the relaxed cervix. It was plain that the inverted uterus was, for every important reason, an inversion involving only the body of the organ. When replacement of the inverted uterine body was attempted, difficulty was experienced. The vagina and cervix uteri became elongated, under the replacing pressure, to a remarkable extent, and the hand and arm were buried deep in the vagina before replacement was commenced. It was effected with considerable difficulty. No disease was observed in the uterus.

Regarding this case a complete satisfactory history was obtained. The inversion was produced while the parts were under no handling or force from without the body. It appears to me to have been one of the kind called spontaneous passive inversion. Bleeding probably first occurred, distending the uterine cavity; then came vomiting, which was probably the source of the injurious pressure which introverted the organ, and simultaneously expelled its contained accumulation of blood-clot. There was nothing in the symptoms or post-mortem appearances to indicate spasm of any part of the uterus, nor to indicate paralysis of a part only of the uterine body.

I find, in my notes of this case taken at the time, the remark that it was evidently best to replace by pressure on the middle of fundus, reinverting that part first.[1]

Case II.—Mrs R., a fine healthy young woman, came to town for her confinement in November 1865. She had already had two healthy children at the full time, the last born about two years before her present lying-in. Pains commenced about 3 A.M., and a well-developed female child was born at 7.10 A.M. There was no hæmorrhage immediately or soon after the birth of the child.

The completion of the third stage was waited for, and the uterus kneaded, but the placenta did not show an edge at the os uteri. The cord was gently pulled, to ascertain if the placenta was free and removable, but it was found fixed. Presently, and now forty-five minutes after delivery, the patient complained of intense desire to make water, and of no other pain. A careful examination was immediately made. The uterus had disappeared from the hypogastrium, and only a small hard part could be felt low down on the right side. The finger, exploring per vaginam, discovered a broad flat surface, in dimensions like the top of a fœtal head. It was firm, and constituted by the fœtal surface of the placenta, the cord being inserted near its centre. Before I had had time sufficiently to examine, the firmness of this projecting mass led me to suspect it was a descending tumour; but this idea was soon dispelled, and the inverted uterus fully made out. The cervix was not inverted, and was high up; its external os not readily discovered, and considerably above the level of the fundus uteri.

The uterus was easily reinverted, and the everywhere adherent placenta artificially separated. A little bleeding followed the separation, but was soon checked by only moderate uterine contraction, and it did not recur.

Intense rigors followed complete delivery. She made an uninterrupted good recovery.

This case appears to me to be an example of artificial active uterine inversion. Pulling at the cord commenced the inversion of the paralysed placental insertion. That this part was paralysed, or very inactive, is proved by the continued attachment of the healthy placenta. Then the body of the uterus, exclusive of the placental site, contracted and seized the central portion now projecting inwards, and expelled it, completing the inversion, and bringing on the feeling of strangury. In this case, replacement was so easily effected that no lesson could be derived from it as to the best method of proceeding.

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[1] This is not to be regarded as anything more than the expression of an opinion founded on an individual observation. On this matter, see M'Clintock, *Diseases of Women*, p. 79.

CASE III.—Mrs M., æt. 24, admitted into the Royal Infirmary October 1871, has been married for two years, and has had one child about a year ago. She is very weak, pale, and anæmic. Complains of continuous loss of blood per vaginam, increased at what she regards as the monthly periods and by going about.

She had enjoyed good health till the birth of her only child. After this event, red discharge did not cease till February, and then it continued absent for three months. When it reappeared in May, she consulted a doctor, who told her that her womb was displaced, and attempted to replace it. She had been attended by a careful medical man, but only now was the nature of her case discovered. Previously it had not even been suspected.

The uterus was small, pear-shaped, deep red, easily made to bleed, not very sensitive to touch. It was entirely inverted, or nearly so, there being only a narrow rim of the lower part of the cervix in its natural position; but the entire cervix was easily replaced.

On 30th October, reduction by forcible taxis was attempted in vain by myself and Dr T. Keith. The patient was under the influence of ether, and the efforts at replacement were continued for an hour and a half. After the operation the patient suffered greatly from feverishness, derangement of the digestive organs, and rheumatoid pains in all parts of her body.

In December I used continuous pressure on the fundus with counter-pressure on the hypogastrium. The pressure was by a cup-shaped pessary attached to one limb of a Santorini tourniquet; while the pad on the other limb effected counter-pressure. To ensure proper adaptation, the instrument required constant watching by an attendant. On the 1st December, after four hours' use of this instrument, I attempted for half an hour to replace the uterus; but in vain. On the 21st December, after continuous pressure by the instrument had been kept up by Drs Hardie and Imlach for twenty-four hours, another attempt at replacement was made, in my unavoidable absence, by Dr T. Keith. On removing the instrument, he found that it had only flattened the fundus. The patient was put under the influence of ether for an hour and three-quarters, and during that time efforts at replacement were continued, and at last crowned by success. Intense rigors, with prostration, came on at once, and she was very ill for a few days; but at no time did her condition excite serious alarm.

There was almost no discharge till the 22d January, when menstruation took place, and was, she believed, natural in all respects. Examination now discovered a patulous condition of the os uteri, but on the whole a healthy condition of the pelvic viscera. Especially, there were no adhesions or other source of fixation of the uterus, and its cavity was of normal length.

She was dismissed on 29th January.

CASE IV.—Mrs E., aged 24, admitted into the Royal Infirmary

28th April 1876, is the mother of two children. Her last child was born five months before her admission. The delivery was natural, so far as she knows. Only, she had a flooding an hour after it, and then the placenta was removed. She continued, she says, to lose blood for nine weeks, when some medicine prescribed by Dr M'Indoe arrested this flow. Ten weeks ago, she had rigors and loss of blood; and she then weaned her babe. Since then she has twice menstruated profusely, and has had occasional bleedings at other times. The condition of the uterus had only recently been suspected and made out.

The inverted uterus is found occupying the upper part of the vagina. It is oviform and about the size of a hen's egg, somewhat tender, smooth, and deep red in colour. Its mucus is slightly alkaline, and its epithelium is rounded and somewhat tesselated. The neck of the tumour is a little smaller than the body, and corresponds to the internal os uteri; but, by pulling on the tumour, the whole cervix can be inverted. After this is done, the cervical portion is easily replaced.

On 1st May, the patient being under the influence of chloroform, a strenuous attempt was made for about an hour to replace the organ by forcible taxis, but in vain. The organ could be forced within the cervix, the latter being elongated, but no farther. Besides the hand, a repositor, consisting of a stick with a cuplike end, was used with apparent advantage. After this operation she was very ill for two days with pain and sickness and vomiting. The latter was attributed to the prolonged use of chloroform; and, in subsequent operations, ether was substituted for it, with the desired result of avoiding the extreme prostration and vomiting.

On 23d May, an hour was devoted to most vigorous forcible taxis, and on 30th May, an hour and a half; but in vain. The body of the uterus could only be pushed within the cervix. On 1st July the efforts were repeated, and continued for three and a half hours. Pressure was for the most part effected by the repositor, but, occasionally, the hand was used by myself, Drs T. Keith, Stephenson, Underhill, and Cotterill, the whole power of the arms being exerted on pressure and counter pressure.

On 2d August, in the presence of several foreigners, I incised the uterus and effected a partial reposition; which subsequently became spontaneously complete.

The uterus was seized by the fingers and pulled down. The cervix became completely inverted and did not descend to the vulva, but came very near it. On the anterior surface of the organ, near the internal os uteri, a vessel apparently about the size of a small surgical probe spouted gently, rising like a large pin's head. The uterus was now incised on its anterior surface by bistoury, the incision extending from just below the internal os uteri to a little above the middle of the body of the uterus, and being carried as deep as could be safely done without injuring the peritoneum; say, nearly a quarter of an inch. When the incision was held open, blood flowed freely from several openings, and somewhat jet-like. Now a similar incision was made in the posterior wall of the uterus. The organ was then treated in the usual way by forcible taxis with the hand. The body was first pushed within the cervix; but now, further pressure further replaced it, but with sensible free laceration of tissue, no doubt beginning at the wound or wounds. The replacement was effected as far as the incisions extended, but no farther. The peritoneum did not give way so far as I could judge, and the subsequent history of the case renders this almost quite certain. The fundus uteri could be felt as a shallow cup whose bottom the finger—covered, of course, by intervening textures—could reach from above. But its shape and its newly-acquired extreme freedom of movement, in consequence of the lacerations, rendered it very difficult of management with a view to reposition. Desperate attempts were made, but in vain. To this difficulty the only moderate dilatation of the cervix uteri partly contributed.

After this operation the woman was not nearly so ill as after the first attempt at replacement under chloroform. Vaginal lotions of carbolic water (1 to 50) were used very gently twice daily. There was no noteworthy bleeding after the operation, and during her recovery there was only scanty purulent discharge.

On 11th August the inverted fundus could still be felt within the cervix. The next examination was made many days afterwards, and the organ was found all right. On the 9th of September she left the hospital. In November she returned to show herself, and was found very well. At no time were there any uterine adhesions or other source of fixation of the womb. The uterus was mobile and of ordinary shape, admitting the probe two and a half inches.

Case V.—M. A. R., æt. 35, unmarried, mother of one child born fourteen years ago, was admitted to the Royal Infirmary in July 1876. She says she had an easy confinement, that the afterbirth came away of itself, and that when her medical attendant left her she felt well. Subsequently, however, on the same day, she suffered severely from vomiting, pains, and inability to pass water. Her urine was not drawn off till two days had elapsed, and the catheter had to be regularly used for this purpose for three weeks; and during all this time she had great losses of blood. For eleven years subsequently she had either losses of blood or a white discharge. During the last three years menstruation has been more regular, but she has felt exhausted and unfit for her work as a domestic servant. Her disease was diagnosed soon after her confinement, and several attempts have been made to cure her. She is weak, pale, and anaemic.

The condition of the uterus was found to be exactly as in the former case (IV.) in every respect; only, when pulled down by the hand, it descended further—that is, beyond the vulva; and not only the neck of the organ, but the upper part of the vagina became inverted. Several energetic attempts to replace by forcible taxis were made in vain, the hands being used alone for pressure and counter-pressure, or the repositor taking the place of the internal hand. From 8th August to 18th September the taxis was well applied by Dr Church every day for from twenty minutes to half an hour, but without any change. The body of the uterus is easily, after a time, returned within the cervix, but no part of the body becomes replaced. On 19th September the patient was anæsthetized, and I incised the uterus as in the former case, continuing the incisions, however, higher up or to within half an inch of the upper margin of the fundus. Most powerful efforts were immediately thereafter used to replace by forcible taxis, but in vain. From the operation she subsequently suffered little, making a good recovery.

On 19th October I proceeded to remove the organ. After pulling it by the volsella down to the vulva, I transfixed the cervix close to the internal os uteri by the ordinary instrument for hæmorrhoids carrying a loop of strong twine. The cervix was now ligatured on each side with all the tightness my hands could produce. Then I cut off the uterus by bistoury about a quarter of an inch above the seat of ligature. Previous to cutting off, the cervix was seized by the volsella below the ligature, to prevent premature retraction. The surface of the incision was whitish, like a section of cartilage, and several vessels bled freely. The hæmorrhage was, however, easily commanded by compression by the volsella. The cut surface was cauterised by a red-hot steel cautery, and the tendency to bleeding arrested. In the middle of the section the Fallopian tubes could be seen and pulled out for a little, not being fixed by the ligatures. The cervix was now returned with the volsella affixed. The woman was made comfortable. The volsella was removed in about two hours after the operation, and the woman placed in bed at 3 P.M. There was no bleeding. Severe sickness and retching, which produced pain in the lower part of the belly, came on immediately after the operation and lasted for two days. At 8 P.M., pulse 100; temperature 98°. Urine drawn off.

*20th October.*—I removed the ligatures which had slipped over the stump of cervix. Noon: pulse 108; temp. 97·8°. Has very little pain or tenderness. Passes her urine.

*21st October.*—Has had a good night. Abdominal pain less. Noon: pulse 104; temp. 100·2°.

*22d October.*—Patient's condition favourable. Temp. 99·8°.

*23d October.*—Patient well in the morning. About 9 A.M.—Temp. 99°. Soon after this a rigor, which was duly treated by Dr Church. 11 A.M.—Pulse 134; temp. 100°. At 9 P.M.—Pulse 100; temp. 99·8°; resp. 18. After this she appeared to get on very well till

*28th October.*—Morning: Pulse 100; temp. 99·8°. Pain in left groin. Some tenderness of abdomen. Considerable discharge of pus per vaginam, which was, as on one previous occasion, washed out with tepid carbolic lotion. Evening: Pulse 118; temp. 102·8°. From this time she became progressively worse, and died on the 29th.

This case was probably one of spontaneous active uterine inversion. The fatal event I am disposed to attribute to an attack of severe and general peritonitis, connected with the effusion of blood between the bladder and rectum, which had undergone decomposition. This effusion of blood appears to me to have taken place, probably soon after the operation, from the Fallopian tubes which had become spontaneously retracted from the embrace of the ligature. Had I foreseen this casualty I would have fixed the cut ends of the tubes to the edge of the wounded cervix. I may add that the cervix had spontaneously become replaced, except a small tongue of tissue mentioned in the account of the autopsy.

*Autopsy by Dr Wyllie, 31st October.*—The body well nourished. The peritoneal sac contained about one pint of a dirty grey purulent fluid, and the bowels were coated with recent lymph, by which some of the intestinal folds were matted together. Within the pelvis, the fold of Douglas was filled with a tarry blood to the amount of three ounces. This blood was limited to the pelvis by the adhesions of the folds of the ileum, the sigmoid flexure, and the great omentum, to each other and to the upper part of the bladder, rectum, and the wall of the pelvis at its brim. Within the cavity of the pelvis, the space containing the blood was lined with a layer of tough laminated blood-clot, about half an inch in thickness. This layer covered the posterior surface of bladder, anterior surface of rectum, the upper part of the vagina, and remains of the uterus. On scooping out from the pelvis the whole of the viscera, and removing them for examination, the following was found to be the condition of the parts:—

1. The body of the uterus was entirely absent, having been removed by operation at its point of junction with the cervix. 2. The cervix was about three-quarters of an inch in length, and the external os was quite patulous, admitting the finger easily. Into the upper part of the cervix there projected downwards from the posterior wall a small leaf-like remnant of the uterus, about three-quarters of an inch in diameter and a sixth of an inch in thickness. The upper extremity of the cervix was covered over and shut off from the abdominal cavity by the layer of blood-clot above described; but when this was removed, the finger could be passed from the vagina through the cervix into the peritoneal sac.

The liver weighed 3 lbs. 15 oz., and was natural. The spleen was large, weighing 1 lb. 2 oz.; its substance was firm.

The kidneys weighed 7 and $6\frac{1}{2}$ oz. respectively. They were exceedingly anæmic, and rather œdematous. They contained a number of small serous cysts.

The heart was natural, except that its chambers and auriculo-ventricular orifices were slightly dilated.

The lungs were anæmic, but otherwise natural.

Although I consider myself very forturrate in having met with so many cases of complete uterine inversion, there is no subject for whose satisfactory study I desiderate more intensely farther experience. This leads me both to bring my concluding remarks within narrow limits, and to speak on some points with diffidence.

On one point I feel quite assured; namely, that the uterine cervix has no important part to play in this disease—that complete inversion of the uterus is a condition of the body of the uterus, and that the condition of the cervix is of trivial import. It may be inverted, or it may not. If it is inverted, it is easily replaced, and its replacement does not facilitate the replacement of the body of the organ. This statement is fully illustrated and confirmed by the cases I have just recorded. It is also illustrated by the most ordinary experience in confinements; for in them, inversion of the cervix[1] is a very frequent occurrence, and one to which I know no case or circumstance lending importance. In most writings on inversion,[2] even the os uteri (externum) is considered as an injurious constrictor. This I hold to be an error, and I indeed exclude the whole cervix from having any place of importance in the disease.

It is well known that mistakes in the diagnosis of this displacement, even when uncomplicated, are not rare, and sometimes very disastrous in their result. But there is in simple cases no difficulty about it, and errors arise from hastiness, or from not suspecting the condition.

The treatment of the disease is in a most unsettled state, which is the natural result of its difficulty and of its rarity. The principal methods of replacement may be included in three divisions:—first, that by long-continued gentle pressure; second, that by forcible taxis; and, third, that by operation. Extirpation is an evasion of the difficulty.

So many cases have been and continue to be recorded of success by long-continued gentle pressure, that strong expectations may be justly entertained of a regular treatment on this principle being duly formulated, at least as applicable to a large proportion of the cases that occur. No doubt, cases of spontaneous reposition are illustrations of this principle of treatment. It is my opinion that, in these spontaneous cases, the chief agent has been negative abdominal pressure, or great retentive power; and it will probably be found that the successful cases of artificial reposition on this principle are cases where there is already negative abdominal pressure, requiring only little assistance from the artificial pressure supplied by the practitioner. It is for the medical physicist to make out whether or not he can produce this favourable condition of the abdomen when it is not naturally present. Even now we can produce it by certain positions of the body—such as on the knees and face;[1] and it is yet to be made out what curative results may be expected from it in cases of inversion and the like, such as ordinary hernia. Artificial pressure is applied, or proposed to be applied, by the colpeurynter, and by various forms of pessaries. It appears to me quite certain that pressure, if duly and long enough applied, will replace the uterus; but any one who has seen cases will need no aid in recognising the difficulty of applying pressure duly and long enough. The reposition of the persistently inverted fundus in Case IV. is probably an illustration of this principle of treatment, the fundus having become replaced by the pressure of the healing and contracting body of the organ.

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[1] *Contributions to the Mechanism of Natural and Morbid Parturition.* By the Author. P. 285.

[2] See Barnes' *Clinical History of the Medical and Surgical Diseases of Women.* P. 734.

Many cases have been reduced by forcible taxis. This method succeeded in Case III. Having regard to the frequent failures of this method under the most energetic and persevering attempts, and having tried in vain to imitate it experimentally in the amputated uterus of Case V., I am constrained meantime to believe that, when it succeeds, the uterine tissue is torn, much as it was cut in my cases treated by operation. My Case IV. strikingly illustrates and confirms this view, for the persistent inversion of the fundus, after the replacement of the body, was no doubt due to the circumstance that the incisions were not carried high enough, and that tearing beyond the incisions was not produced by the replacing efforts of the practitioner.

In conclusion, I have only a few remarks to make on this operation. It is evident, from what I have already said, that, not regarding the cervix as the source of difficulty, I do not expect direct advantage from cutting it. Incisions of it have been repeatedly proposed and performed, and it is my opinion that they may in some cases be useful, but only indirectly. They may permit easier access to the hand manipulating the really inverted part, the body of the organ; or they may facilitate the laceration of the body on the well-known principle that it is easier to continue a tear than to commence it. In my operations, the body of the uterus was incised. In Case IV. the incisions were evidently enlarged by laceration while the organ was being replaced.[2] In Case V., which was of fourteen years' standing, the incised organ could not be induced to give way and tear. It is very probable that, in the successful cases, the peritoneum escapes laceration.

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[1] *Researches in Obstetrics.* By the Author. P. 430.

[2] See some pertinent remarks by M'Clintock, *Diseases of Women*, p. 94.

## ARTICLE II.—*Note on Esmarch's Operation for the Relief of Permanent Closure of the Jaws.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

CASES of permanent closure of the jaws are most troublesome, and, with a few exceptions, most unsatisfactory in regard to treatment.

The causes of such permanent closure may be, anchylosis of one or both articulations, the growth of tumours in or near the jaws, or the formation of an osseous band or arch uniting the upper and lower jaw; but the most common cause of this condition is the cicatrization and contraction which result from necrosis of the jaw-bones, or from ulceration, sloughing, or wounds of the gums, mucous membrane, and soft parts of the cheek.

Cases of this condition vary in intensity, according to the extent of the tissues involved. Adhesions or cicatrices may exist on both sides of the face, or on one side only, but in all cases the jaws are more or less fixed, and in aggravated examples are perfectly immovable, so as to prevent mastication and interfere with proper speech.

In closure of the jaws depending upon anchylosis of the joints, the forcible breaking down of adhesions may succeed in relieving the condition; and if not, the division of the lower jaw through its neck or ramus, as suggested by Dieffenbach, will usually improve mobility; while in cases depending upon the growth of tumours, or upon osseous union, the removal of the growth or osseous band will certainly free the jaws if no other complication exists.

It is, however, to the more common cases, the result of cicatrization and contraction of the soft parts of the mouth and cheek, to which I wish principally to direct attention. Many of these cases are complicated with loss of substance of the lips and cheek; and although such loss of substance may be remedied by one or more plastic operations, experience has proved that operative and mechanical interference with the cicatrices and contraction, causing the closure of the jaws, is rarely attended with much success, as recontraction speedily takes place. In a few instances, combined operative and long-continued mechanical treatment directed to the cicatrices has given relief, but few patients will submit to the prolonged suffering which such a proceeding requires.

If cicatrices and contraction exist on both sides, I do not think that any treatment will be permanently successful in relieving the closure of the jaws unless some mechanical means be continuously employed; but when the condition is confined to one side only, we have in the ingenious suggestion of Professor Esmarch a successful method of relieving the condition and obtaining mobility on one side of the jaws. Professor Esmarch had under his care in 1854 a case of closure of the jaws, resulting from the contraction of a cicatrix in connection with necrosis of the lower jaw. The portion of necrosed bone was in front of the cicatrix, and when it was removed, a false joint formed at this point, and mobility of the jaw on the opposite side resulted.

This case led Esmarch to suggest in 1855 that, in similar cases of closure of the jaws, a piece of the entire thickness of the lower jaw should be cut out immediately in front of the cicatrix or contracting agent. This suggestion was successfully carried out in 1858 by Dr Wilms of Berlin, and afterwards by Dittl of Vienna, by Wagner of Königsberg, and has also been successfully performed in this country by Mr Mitchell Henry, Mr C. Heath, Mr Bernard, and myself.[1]

Rizzoli of Bologna has suggested and successfully practised a similar proceeding, but he advises that the bone be simply divided, and no portion of it excised.

The following is a short note of the case in which I successfully adopted Esmarch's operation:—

David J., æt. 9, was admitted into my wards in the Royal Infirmary on the 4th of last July. Four months before admission he was attacked with necrosis of the upper jaw, after suffering from some form of fever. When admitted, the left angle of the mouth and surrounding portions of the upper lip and cheek were seen to be destroyed by ulceration, and a portion of the alveolus of the upper jaw was exposed. The soft textures around this exposed portion of bone were drawn in, and were adherent to both upper and lower jaws, which were fixed together by a broad and firm cicatrix. The jaws could only be separated to the extent of about an eighth of an inch. On the 6th of July, the necrosed portion of bone, which consisted of the greater part of the left upper jaw, was seized with forceps and removed. On the 31st of July, an attempt was made to fill up the gap in the soft parts, and, at the same time, to relieve the closure of the jaws. This proceeding consisted in cutting out the dense cicatrix and sliding forward a flap from the healthy portion of the cheek, and forcibly separating the jaws by means of a screw gag. The operation was only partially successful in relieving the deficiency in the cheek, and a few weeks after, notwithstanding the use of mechanical means, the jaws were as firmly closed as ever. On the 22d of August I performed a second plastic operation, and on this occasion put in practice an ingenious method, the suggestion of Professor Estlander, of the University of Finland. This method consists in taking a flap from the lower lip and turning it round so as to fill up the deficiency in the upper lip and neighbouring cheek. In this operation, the labial margin, together with the coronary artery, is not cut through. Should the deficiency of tissue be in the lower lip, Professor Estlander advises that a corresponding flap be taken in the same way from the upper lip, and turned round so as to fit into the gap in the lower lip. At the same time I again freely divided the contracted tissues uniting the jaws. This second plastic operation was perfectly successful in making good the deficiency in the soft parts; but very shortly after, the jaws became again closed, as formerly. On the 16th of October, it was found that the contraction of the cicatrix was more aggravated than ever, and there was very slight mobility of the jaws, the patient only being able to take the softest food by pressing it between his teeth into his mouth. On the 18th of October, I performed Esmarch's operation, and removed a portion of the entire thickness of the lower jaw immediately in front of the cicatrix, and about one inch to the left of the symphisis. The portion of bone excised measured three-quarters of an inch in length, and it was removed by means of a fine narrow saw, through an external incision carried along the base of the jaw. A small pad of lint secured to a piece of strong silk-thread was placed between the sawn ends of the bone, and for it was substituted in a few days a piece of guttapercha, with the view of promoting the formation of a false joint. No trouble followed the operation, and within a week he was masticating bread and other solid food with the unaffected side of his jaws. Two months after the operation, he could masticate any kind of solid food easily, and the jaws could be readily separated to the extent of three-quarters of an inch.

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[1] See Heath on Injuries and Diseases of the Jaws, 2d Edition, page 340, for latest account of this operation.

The one imperfection of this operation is that it only results in mobility of the jaws on one side; but if it even succeeds in doing this, it places the patient in a much better position than if the entire extent of the jaws was fixed. After the performance of this operation, it is important to regularly and freely move the lower jaw, so as to prevent any chance of union of the bone where excised. The introduction of a piece of guttapercha or other body between the ends of the bone for some time after the operation was suggested by Rizzoli, and is an additional means of preventing union. The plastic operation of Professor Estlander is a very useful one where it is necessary to repair deficiencies of tissue in the upper or lower lips. It has the advantage that it saves the labial margin of the transposed flap; and although it leaves a small and somewhat one-sided mouth, this latter condition can be remedied, as I have proved in another case operated upon lately, by a second slight operation, so as to extend the mouth on the contracted side.

## ARTICLE III.—*The Operative Treatment of Genu Valgum.* By ALEXANDER OGSTON, M.D., Surgeon to the Aberdeen Royal Infirmary.

THE following history illustrates, in a bad case of knock-knee, a means of treatment hitherto untried, but which yielded on both legs results so perfect as to warrant its being recommended in similar cases.

The patient, D. G., a male, aged 18, was admited into the Aberdeen Infirmary on the 19th of April 1876. His occupation was that of a moulder in an iron foundry, and his account of the commencement and progress of his disease was as follows:—When six years of age, he had an attack of typhus fever, followed by general weakness, most marked in his lower limbs. After a time he noticed a deformity in his knees, which became bent inwards, and the deformity has been steadily increasing up to the present time. Until his tenth year he remained pale, delicate, and weak, but with no definite ailment save in his knees, and without any noticeable deformity elsewhere. Since his tenth year his general health has been very good.

*Appearance on Admission.*—His general appearance is that of a robust, strongly muscular, and well-nourished lad, but whose growth has been stunted. The deformity of his lower extremities immediately attracts attention. When upright, or lying on his back with the limbs straight, the knees fall inwards so as to knock against each other, and one knee has to be placed behind or before the other, to enable him to stand. If his feet are placed together, the right knee crosses completely over to the left knee, and *vice versa*, so that a hand can be passed through between the transposed knees. When he walks, he drags the posterior knee from behind its fellow, makes it circumnavigate the latter, and finally come entirely in front of it, before the weight of the body is transferred from the one to the other. The normal slightly outward direction of the tibia is so exaggerated that, when lying on his back, the right leg, in the extended position, forms an angle of 150° with the right thigh, and the left leg an angle of 160° with its thigh. When standing the deformity is greater, owing to the superincumbent weight. In each femur the internal condyle is much elongated, while the external condyle is of normal length, but has its anterior surface so far absorbed that the patella is no longer retained in position, but is displaced outwards, and rests on the outer surface of the femur, with its inner and outer edges directed respectively forwards and backwards. Both patellæ, especially the right, can be pressed inwards to near their normal positions, but immediately slip outwards again. Both lower limbs seem shorter than normal, and on standing the biceps tendon is abnormally rigid and prominent in each.

When the knees are flexed to somewhat more than a right angle, the deviation outwards of the legs completely disappears, and it is easy to recognise by sight and touch that the internal condyles are elongated to between half and three-quarters of an inch. In this position, too, the patellæ are drawn still further backwards, so that they come to rest, not on the femur, but on the outer tuberosity of the tibia; and on the front of the femur the groove between the two condyles can be felt.

There exists a knotty thickening of the lower extremities of the tibiæ, fibulæ, radii, and ulnæ. The clavicles are unusually curved. The upper part of the sternum is very prominent, and the anterior ends of the ribs are enlarged close to their articulations with the costal cartilages. The spine is somewhat excurved in the lumbar region. The health is otherwise perfect.

This deformity was treated for some time by splints and bandaging, until it became evident that such means were utterly futile, and that nothing but an operation could straighten the limbs.

In a somewhat similar case, Mr Annandale had recently, in the Edinburgh Infirmary, opened the joint and sawn off the elongated part of the internal condyle under carbolic spray and antiseptic precautions. In the above case, however, it was decided to avoid the large opening into the joint, and substitution of a sawn surface for a smooth cartilaginous facet (both unavoidable in Mr Annandale's operation), by making subcutaneously an oblique division of the elongated condyle, fracturing it off as it were, so that it might be pushed higher up into its normal relation to the outer condyle, and there allowed to unite. It was plain that this should permit the limb to become at once straight, since the elongation of the condyle was, as it were, the essence of the deformity. The following operation was therefore decided upon, and put into practice, on the 17th May 1876, on the left knee:—

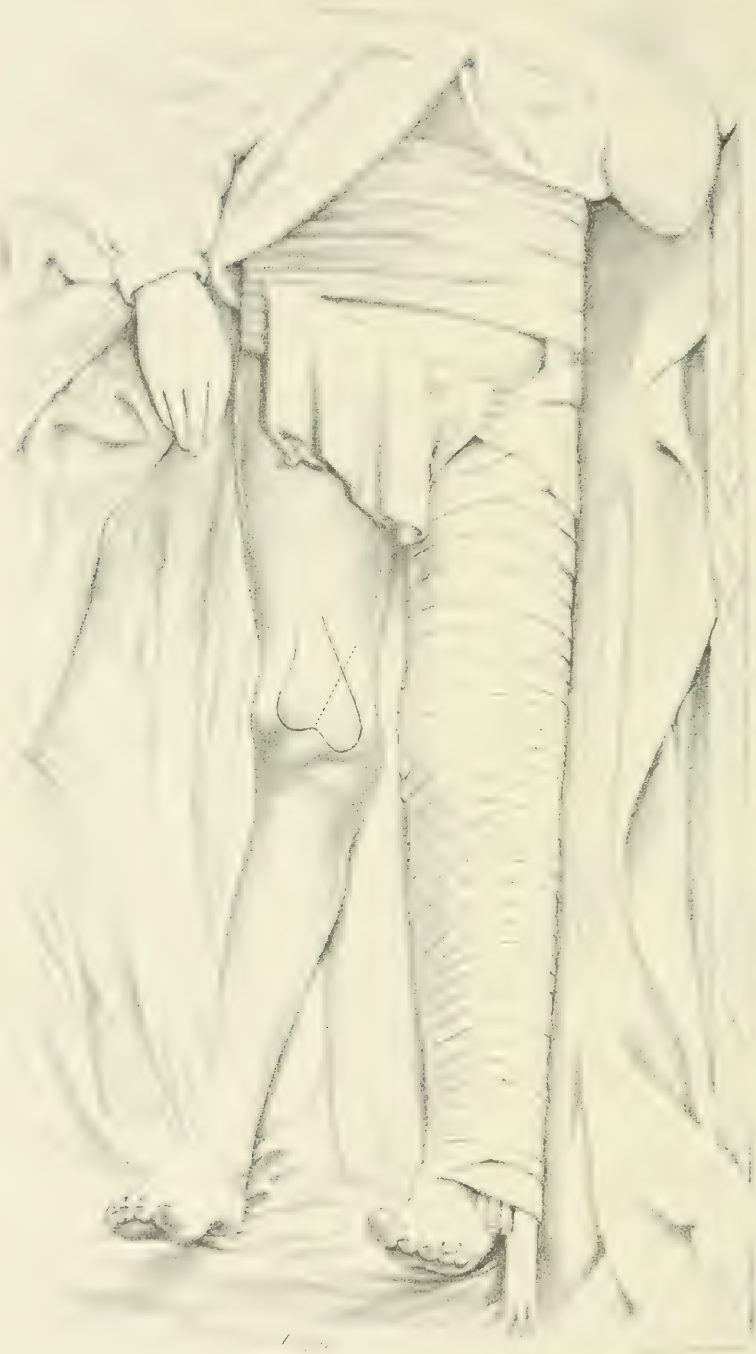
The patient was chloroformed, the knee flexed as far as possible, and the thigh turned outwards. A long and strong tenotomy knife (Adams's) was introduced through the skin, three and a half inches above the tip of the internal condyle on the inner side of the thigh, and so far back as to be opposite the ridge of bone running between the linea aspera and the condyle. Its blade was carried forwards, downwards, and outwards, over the front of the femur, with its cutting edge directed to the bone. When its point could be felt under the skin, in the groove between the condyles where the patella would normally have been lying in the flexed position of the limb, the cutting edge was pressed against the bone, and the soft parts and periosteum divided by one slow firm movement in withdrawing the knife. The external wound thus made was about one third of an inch long, and formed the entrance to a subcutaneous tunnel, running obliquely over the front of the femur, and ending in the cavity of the joint. Adams's saw for subcutaneous division of the neck of the femur was introduced into the tunnel, and the condyle sawn off by directing the edge of the saw straight backwards. The position of the saw could be exactly controlled by feeling its point working gradually backwards in the groove between the condyles. As soon as it was estimated that the condyle was almost entirely separated, and that the saw had arrived near the popliteal space, the saw was withdrawn. The knee was completely extended, and then with the hands, and the operator's knee as a fulcrum, the patient's knee was forcibly straightened by bending the leg inwards. The remaining connexions of the condyle with the femur gave way with a crack on the application of very moderate force, and instantly the leg became as straight as a healthy limb, and could even be put into a somewhat bandy (genu varum) position. *The whole of the operation was conducted under carbolic spray, with a minute observance of Lister's antiseptic precautions, and the limb was put up in antiseptic dressings.* It was than simply bandaged to a Liston's long splint, as in the drawing, the thigh and leg being kept in the same line by pads, and the boy was placed upon a hard mattress.

The reaction was almost *nil.* His temperature, carefully taken, never rose above 99·8°. The joint, at first filled with blood, never became hot or tender, and on the 2d June the splint and bandages were discontinued and passive motions commenced.

On the 6th June, the other knee was operated on in the same manner, with an equally favourable result, the temperature never rising above 100°. The splint and bandages were removed on the 21st June, and passive movements commenced.

The movements were continued on both limbs for a month, and were at first attended by crackling and grating, as if from rough or displaced cartilages, whenever the knees were flexed to a right angle. There was never any pain, and the movements became steadily smoother until, on the 9th July, seven and a half weeks after the operation on the left knee, and less than five weeks after that on the right knee, he was allowed to rise and walk. The movements speedily became normal, and he was dismissed, walking perfectly, on the 21st July. The patellæ were even then tending to assume a more normal position.

I am indebted to Mr James Gordon, student of medicine, Aberdeen, for the beautiful sketch of the boy after the first knee had been straightened.



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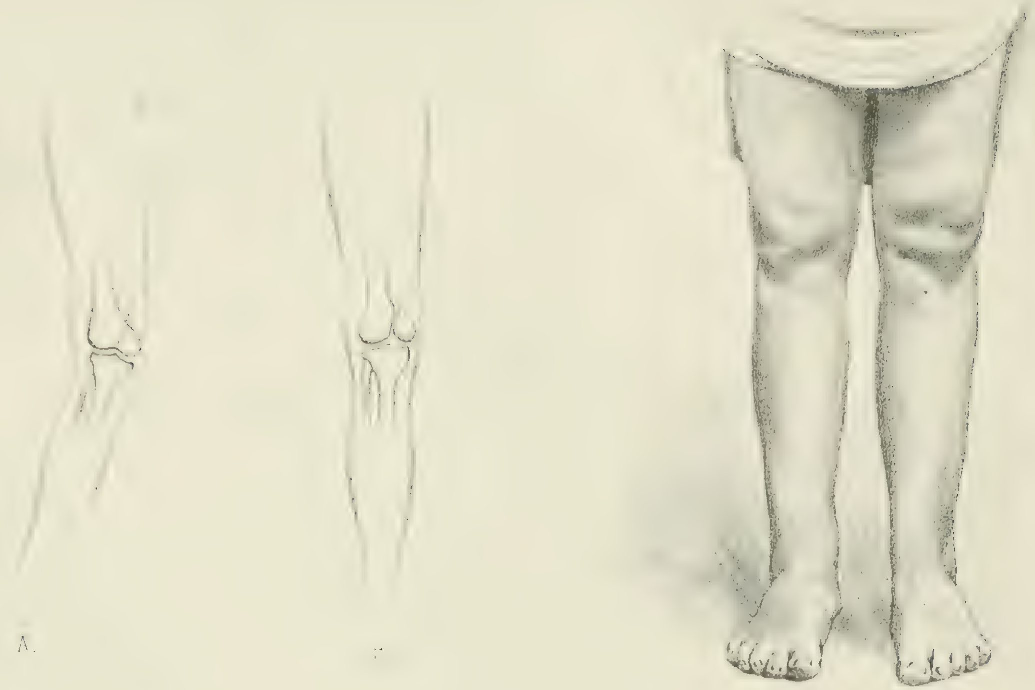

GENU VALGUM.

A. Line of Subcutaneous Division. B. Position after operation. C. Case 3, a Photograph of the patient when cured.

## ARTICLE IV.—*Case of Badly-united Fracture of the Femur treated by Sawing across the Bone at its Angle, with Antiseptic Precautions.* By JOSEPH BELL, F.R.S. Ed., Surgeon to the Royal Infirmary.

MASTER J. M., æt. 12, sustained, on 19th October 1875, a severe comminuted fracture of his left femur, about four inches below the hip-joint, from a horse falling on him. He was placed in the hands of a well-known bonesetter in the east of Fife, who was summoned by telegram, and who undertook the case on his own responsibility. I am informed that he treated it by bands and splints, without any precaution to prevent movement of hips or knee-joints; and in a soft bed, which allowed the patient's hips to sink into a hollow. He did not examine it for eight weeks after accident. The treatment extended over ten weeks, and during the greater part of this time the patient suffered very severely from pain, feverishness, and sleeplessness.

State of Master M. when first seen by me on 30th June 1876:—

His left lower limb was shortened to the extent of five inches; he had a high-heeled boot, but even with its aid walked with great difficulty. The foot was everted, pelvis twisted, and back had a compensating curve. On stripping him, I found the deformity to depend on a fracture of the left femur, which had probably been comminuted, but which had united, deformed both by an angular displacement, and also by a rotary displacement. The upper end of the enormous mass of new material which mended the fracture was about 4 inches from the hip-joint; the thickened mass of bone was about 2½ inches in diameter at its thickest, and projected as an acute angle outwards and forwards. When the limbs were laid side by side, the shortening in consequence of the angular displacement was about 5 inches; and the lower portion of femur, knee-joint, and leg and foot were much everted. The salient angle, which contained less than a right angle, projected sharply under the skin; but the amount of new bone thrown out behind was very great—so much so as to overlap inwards and backwards the femoral artery, which could be easily compressed against the bone.

The boy's general health was good, but his lameness was very bad, and was getting worse instead of better; and his halting gait and spinal curvature were giving much uneasiness to his parents.

I explained to his relations that it would be necessary to do something, and proposed to attempt refracture, though I explained to them that the length of time which had elapsed since the injury, the immense amount of new bone, and also the high situation of the fracture, would render the operation difficult and doubtful.

They placed the case unreservedly in my hands; brought the boy to town; and on Monday, 14th August, I attempted refracture of the bone, assisted by Dr Grant and Messrs Brown and Scougal. Having put the patient deeply under chloroform, I first tried, with all the strength I could muster, to break the bone across my knee —no effect was produced. I then used Mr Butcher's powerful steoclast, with which I was provided through the kindness of Professor Spence, who lent me his own one, made on Mr Butcher's model. This powerful screw failed to fracture the bone. The angle being such a salient one put the pressure at a great disadvantage; and after long and exhausting efforts carried on as far as I dared to risk the vitality of the parts, we had to give up, absolutely baffled.

I then explained to the parents by letter the necessity for future operative procedure; and on Monday, 21st August, after putting him fully under chloroform, with the assistance of Dr Grant and Messrs Brown and Scougal, I performed the following operation:—

The parts being thoroughly washed and soaked in a solution of 1–20 carbolic acid, I, under the protection of a steam spray, cut down to the angle of the bone, with a strong-backed, straight scalpel, dividing all the tissues, including the periosteum. The incision was rather a stab than a cut, passing obliquely backwards and outwards from a point nearly over the superficial femoral artery down to the bone. On withdrawing the knife I closed the wound with ring finger of the left hand, and maintained the accurate apposition of the edges. Into the wound thus made I then introduced a long, narrow-shaped saw, tapering nearly to a point, and with rapid but very slight movements of it sawed through the bone at its angle as far backwards and inwards as the line of the artery (which I could distinctly feel under my left finger) allowed me. When the bone was thus with some labour nearly divided at its thickest part, I then withdrew the saw, and putting a pad of carbolized gauze over the aperture of entrance, succeeded in fracturing the bone completely across my knee. A little gentle force then soon succeeded in restoring the straightness and nearly the original length of the thigh. The wound was dressed antiseptically, and two Gooch splints, well padded, put on, one on the outside, and the other in front of the thigh. The apparatus for applying extension by weight and pulleys had been already prepared, and the plaster fastened to the leg, knee, and lower third of thigh; so it was at once put on, and we found that about ten pounds of sand kept the limb in admirable position, without undue pressure or muscular fatigue.

The after-progress of the case was so absolutely uneventful as to leave me nothing to record. He never lost an hour of sleep or a single meal.[1] He never had a twinge of pain in the wound. His temperature and pulse remained normal. I dressed it under spray at first daily, then every two, three, and four days; at length only weekly. The wound healed at once, so far as the deeper parts were concerned; and had it not been for a superficial sore, the result of the extreme force used on the first occasion, the dressings would have been very few. On the 14th of October the splints and weights were removed; and at the end of the next week he went home, using a crutch, within nine weeks of this somewhat formidable operation.

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[1] This was in remarkable contrast to the progress of the fracture when first set.

Those who are familiar with current surgical literature and practice will be aware that in the present day there are at least three different plans in common use by which deformed or badly-united bones may be reingrooved:—1. Subcutaneous osteotomy as proposed and practised by Mr Adams, in which a strong knife and a narrow saw are used to divide the neck of the femur in cases of osseous anchylosis of the hip-joint in bed position.

2. Osteotomy, also subcutaneous, by chisel and mallet, as advised by Mr Maunder, and practised by him, Mr Annandale of this city, and others.

3. Section of the bone and resection of the badly-united ends with antiseptic precautions, as advised and practised by Mr Lister.

The plan I adopted in this case seemed to me to unite the advantages of subcutaneous and antiseptic surgery. Being subcutaneous, I could almost ensure against entrance of air; being antiseptic, I could prevent putrefaction; by attention to escape of serum, tension was also avoided.

I thought of using chisel and mallet, but preferred the saw as being more manageable and less likely to risk injury of the femoral artery, which was so much in the way. The only really anxious part in the operation was in the breaking of the last bit of the bone, in case of injury of the vessel.

I need hardly say that the position of the fracture within less than five inches of the hip-joint made it very dangerous, had anything gone wrong in the wound, as a compound fracture of the upper third of the femur is well known to be a frequently fatal accident.

## ARTICLE V.—*On Oil of Turpentine in Sciatica.* By W. ALLAN JAMIESON, M.B., M.R.C.P., Edin.

A MEDICINE which has been employed in the treatment of sciatica from the days of Galen downwards would seem scarcely to need anything further said in its favour; the observations I am about to make are an attempt to define more accurately the cases in which oil of turpentine may be used with most advantage. While our methods of investigating into the nature and natural history of diseases are day by day becoming more precise, and, as a necessary consequence, our knowledge of morbid states of the system, and of the finer shades of distinction between each, is continually growing more perfect, the exact treatment of the varying forms is built up more slowly, and to some extent less satisfactorily. Even in the wards of a large hospital, cases of the same disease presenting a moderate degree of parallelism are not of constant occurrence, so that the experimental method of research into the action of one particular remedy in one special variety of disease, has often to be carried on for years before an induction which is worth anything can be drawn. In private practice long intervals of time frequently elapse between instances of disease similar in kind, while it is more difficult to ensure that the mere surroundings of the patient may not injuriously affect the observations.

Sciatica, in our cold and damp climate, is common enough; it varies with the age, temperament, and diathesis of the person affected, while its treatment as yet is almost entirely empirical. Adopting Dr Anstie's classification, three main groups may be recognised. The first of these is that occasionally seen in young persons, most commonly females, though by no means unknown in men. While the constitution of these patients partakes more or less of the hysteric character, the exciting cause may be often found to exist in some peripheral irritation of branches of the sacral plexus, more especially of those distributed to some part of the sexual apparatus. It is not a common form of sciatica, and I cannot at present recall a very typical instance in my own practice. Perhaps the following case is the best illustration which has come under my notice:—

M. S., æt. 24. Domestic servant. Of a reddish-fair complexion. Has been for more than a year in the service of a family in Edinburgh, who took her with them to an elevated situation a few miles from Dunbar in the beginning of August. For three months before I saw her, on the 19th September 1876, she had experienced pain along the right sciatic nerve. A medical man who attended her soon after it began, gave her temporary relief by subcutaneous injections of morphia. For a time the pain was endurable, and she went on with her duties, but it gradually became worse, preventing sleep, and incapacitating her from work. She was sent in to a sister in town, at whose house I visited her on the date named above. She then complained of severe pain in sciatic notch at knee and outer ankle, but, contrary to what is usually observed, she seemed unable to keep the limb still in bed, and the pain on pressure behind the trochanter was elicited by slight contact. Her evening temperature was 98·8°, and her state otherwise normal. I injected ¼ grain of acetate of morphia deep into the gluteal muscles, and ordered her—

℞. Olei terebinthinæ, ʒij.
Olei ricini, ʒiv.
Mucilaginis, ʒiv.
Aquam ad, ℥ii.

The draught to be taken early next morning.

When seen on the afternoon of the 20th September, the morphia had given little relief, while, though the medicine prescribed had acted freely on the bowels, the pain in the sciatic was as great as ever, and the restless movement of the limb even more so. As her sister was from circumstances unable to wait upon her, she was admitted the same day into the Royal Infirmary, under Dr Haldane's care, where her case proved a tedious one. There was here no history of rheumatism, nor of any such exciting cause as cold or damp, while the age of the patient and character of the pain raised at least a suspicion of an hysteric element, so that the failure of oil of turpentine to afford relief scarcely astonished me.

The features of the second form are well marked. It is especially that connected with the period of tissue degeneration. It is comparatively rare in my experience before fifty, though cases much resembling it are occasionally met with in persons between twenty and thirty. Ten examples, of which I have notes, since 1870, have been exactly divided between the sexes. General experience of all cases of sciatica prove it rather more frequent among men. Pain often commences first in the lumbar and sacral regions, and then after a variable time creeps downwards into the parts more immediately supplied by the great sciatic nerve. Points tender to touch also manifest themselves successively from above downwards. Thus, those which first appear correspond with the terminations in the skin of the posterior branches of the sacral nerves; then the coccygeal, and perhaps the vesical branches, are next affected, and nearly simultaneously with these a fibular point over or near the head of the fibula; and, lastly, an external malleolar. This last is very persistent, continuing even when those higher up in the chain of distribution have almost or altogether faded away,—so much so that I have frequently seen sinapisms and even blisters applied over the external ankle in the vain hope of thus mitigating the pain there. Sometimes a painful point is developed in the centre of the calf. Cutaneous anæsthesia is also a symptom, but is often masked in the early part of the disease by the tenderness of certain portions of the skin becoming a prominent feature during its decline. The motor branches of the nerve are often, indeed, usually affected, locomotion being impaired quite independently of the pain resulting from use of the limb, and consequent pressure on the nerve and its branches by muscular contraction.

The predisposing cause is in many cases long-continued pressure on the sciatic nerve—in females who sit much and take little exercise—in males whose occupation is sedentary, though not necessarily one within doors. I am inclined to think, too, that position in sitting may determine which nerve is affected. Nothing is more common than for an individual in a family to have his or her own seat, by one side or other of the fire it may be. In this way, one nerve is likely to suffer more from pressure than the other; and so, from local anæmia thus induced, or depression of functional activity, to be easily influenced by such an exciting cause as damp cold. Exposure to this latter is usually the immediate precursor of an attack. In one of my cases the seizure was brought on by a long drive in an open gig on a raw cold evening, across a bleak Northumbrian moor, followed by a night spent in a draughty room with insufficient bedclothes. In another, a miller caught an attack by getting wet while assisting to drive a horse and cart out of the Whitadder, and subsequent carelessness in changing his clothes.

While no temperament or diathesis can be said to be free from liability to an attack, the class of persons most subject to this form of sciatica, and who derive most benefit from oil of turpentine, are those who present the following appearances:—

Usually middle-aged; they look older than they ought, and have hair, as Anstie so well phrases it, *prematurely and permanently gray.* The complexion may be pale or fresh, but it nearly always exhibits a peculiar *ashy gray* or *stone-coloured* aspect—a point to which I attach much importance. The proof of tissue degeneration may be more conclusively shown by rigid radial arteries, possibly by arcus senilis.

The following case, which was first seen by me on the 26th August 1870, illustrates many of the particulars stated remarkably well:—

Mrs W., æt. 56, a lady in comfortable circumstances, who had a family of six children, and up till then had not had any severe domestic trial or worry. She had, however, occasionally of late, attacks of the neurotic form of angina, and one of her daughters subsequently had neuralgia of a severe type in various groups of nerves. She was tall and stoutly made, rather pale, her complexion having the ashen hue which I have already mentioned. Her hair became gray early in life, and continued so. She never had gout or rheumatism. I afterwards observed that, when at home, Mrs W. always occupied a large arm-chair, so that her right side was next the fire. On the 11th August 1870 had a smart seizure of choleraic diarrhœa, which lasted all night and left her weak. She was then living on the banks of the Tweed in a rather damp and relaxing situation. On the 22d she began to have pain in the back, and along the course of the right sciatic nerve; three days after she was quite lame, so she returned to her home in Berwick, and I saw her on the 26th.

I found her with a clean tongue, but marked with many deep cross fissures; appetite fair; bowels, now perfectly regular. Pulse 60, equal, of good strength. There was severe pain along the course of the right sciatic nerve, paroxysmal in character, and when the attacks came on, there was also pain in the urethral branches; urine normal as regarded quantity and quality. A blister was applied behind the great trochanter, and rest in bed enjoined.

*29th.*—Pain very intense, felt chiefly in calf and outer ankle. One twenty-fifth of a grain of sulphate of atropia was injected subcutaneously, which produced distinct physiological effect, and afforded temporary relief.

*30th.*—Repeated atropia.

*31st.*—Pain less; 15 grains of carbonate of iron twice a day.

*3d September.*—Pain still continues; 5 grains of iodide of potassium, with 8 minims of tincture of colchicum, were prescribed, to be taken three times a day; and as the nights were sleepless, half a drachm of chloral at bedtime. On the 6th there was some diarrhœa and tormina; the colchicum and chloral were omitted, but the iodide was continued till the 11th, when, as it appeared to be doing no good, it was given up, and the carbonate of iron resumed.

*17th.*—Passed a very bad night; did not sleep, and suffered much pain in coccygeal, vesical, and malleolar branches. Injected $\frac{1}{4}$ grain of acetate of morphia, which relieved, but gave rise to much nausea. 40 grains of chloral at bedtime.

*18th.*—Slept well, but pain undiminished. The chloral draught was repeated, and the following mixture directed to be taken early in the morning:—

℞. Ol. terebinth., ʒij.
Ol. ricini, ʒiv.
Tinct. cardam., co., ʒi.
Mucilag. et aq. ad ℥ii.

*19th.*—The medicine acted freely; the pain is much abated—so much so, that Mrs W. can now lie on her side. The mixture was repeated on the 21st, and again on the 25th. Next day the pain was so nearly gone that she could sit comfortably. A slight degree of numbness persisted for some time; but recovery was uninterrupted and complete, no fresh attack having since occurred.

In this instance the effect of the oil of turpentine was so real, as compared with that of all the other means which had been tried, that my patient, when I visited her after the first dose had acted, accosted me with the exclamation, "Why did you not give me that medicine at first?" Its nauseousness had in fact deterred me, but I learned a lesson not to hesitate in ordering it on that account. Since then, I have employed it as the first remedy in eleven cases, all of them presenting in greater or less degree the characters of the class detailed above. In ten of these, relief from pain was immediate, not in any, entire or complete after a single dose, but such an amelioration as to render lying on the side affected, or even sitting up, quite possible. A second or third, in several even a fourth dose, was required to reduce what was previously intense suffering to a state of slight numbness. In one man it failed to cure, though his case seemed well suited for its employment.

T. S., æt. 61, was seen by me last March, after he had suffered from sciatica in the right leg for some weeks. He was a man of by no means temperate habits, and was employed in a large woodyard. He presented both the gray hair and ashy expression. Turpentine gave him temporary relief, but the pains recurred. Blisters, acupuncture, iodide of potash, iron, and strychnia were tried in succession, without effect, but the pain finally yielded to subcutaneous injections of morphia, at first daily, then every second day. I think in his case there was a rheumatic taint; certainly it was one of the most obstinate I have met with.

Oil of turpentine has been long recommended as useful in some forms of sciatica, but if we are to judge from what one of our latest writers on therapeutics, Dr Phillips, says, that in which it may be advantageously administered has, up to the present time, been obscure. Dr Phillips endorses the opinion of Nothnagel, that the indications for its use are quite uncertain. Sir Thomas Watson suggests that it may be most serviceable in cases where the irritation of the nerve is connected with a disordered state of the kidney, but there was no alteration in the urine in several patients under my care, where, nevertheless, turpentine cured. Dr King Chambers, who has in his Lectures, chiefly clinical, written more fully on the subject than any author with whose works I am acquainted, taking into consideration the proximity of the cæcum on one side and the rectum on the other to the trunk of the sciatic within the pelvis, thinks that while disorder of these viscera may lead to the production of sciatica in some cases, turpentine given either as an enema or by the mouth exerts a direct local action on the nerve. Dr Anstie, in his classic work on Neuralgia, does not once mention turpentine as an internal remedial agent, although in his previously published article in the *System of Medicine*, he briefly alludes to it as worth trying in the more obstinate cases of this disease, in ten minim doses thrice a day. I may remark that I have employed it in this way without any benefit in cases which were at one relieved by a purgative dose. And here I may also combat an objection which may be raised, viz., that the good results obtained from turpentine are due solely to its drastic action; a full dose of compound jalap powder has no effect in moderating an attack of sciatica, however completely it may evacuate the bowels. In this opinion I find myself to be at variance with that of a very distinguished clinical teacher and accurate observer, the late Dr Warburton Begbie. In an exhaustive paper on the Therapeutic Actions and Uses of Turpentine, published by him in the July number of the *Edinburgh Medical Journal* for 1871, for recalling my attention to which I am indebted to the kindness of Dr Muirhead, there occur the following remarks:—"The *modus operandi* of turpentine in neuralgia, and particularly in sciatica, is, I am disposed to think, not unfrequently connected with its action on the intestinal mucous surface, with some irritation of which the painful nerve-affection is not unfrequently connected. In illustration of this I may refer to an experience by no means uncommon, that after a brisk action of a cathartic—and for this purpose none is more suitable than the combination of turpentine and castor-oil—a severe attack of sciatica has been entirely removed."

Turpentine belongs to the narcotic stimulant class of remedies, and I agree with Dr Chambers in thinking it acts locally, just as its action is also probably local on the unstriped muscular fibres of the intestines when, administered in the later weeks of enteric fever, it so rapidly and effectually reduces the tympanitis so distressing and dangerous. Turpentine, then, to do good in the sciatica of the period of tissue-degeneration, must be given in doses of at least two drachms, combined with half an ounce of castor-oil, suspended in mucilage, and rendered more palatable by the addition of an ounce or so of cinnamon-water. The draught is best taken early in the morning. The dose generally acts powerfully two or three times, but its action is not followed by exhaustion at all commensurate with its effect on the bowels. Sometimes there is considerable difficulty in keeping the mixture down; usually, however, this is less urgent when cinnamon-water is employed as the vehicle. In the treatment of all forms of sciatica, absolute rest in bed must be maintained, not only while the pain continues, but for some time after it is gone. This is oftentimes difficult, as patients rebel against what they consider an unnecessary restriction. The following, among others, is an instance where I had reason to regret the neglect of this precaution.

J. B., aged 25, a masculine-looking female, an outworker on a farm in Berwickshire, began to suffer from sciatica in February 1875. I saw her on the 20th March, when the pain was so severe as to confine her per force to bed. I ordered her the turpentine draught, and on 24th March received a letter stating that this had at once relieved her, and she had gone to work. She sent for me on the 28th, and was found worse than ever. The turpentine was repeated on the 29th, and on the 1st and 2d April, and this time, warned by her previous experience, she rested longer, and got quite rid of her troublesome enemy.

Commonly the dose does not need to be repeated oftener than every third or fourth morning. I have never seen strangury produced by its use in sciatica, nor indeed any unpleasant consequences.

Lastly, there is a third group of sciaticas, in which the sheath of the nerve probably suffers from the action of a specific blood-poison, be that gouty, rheumatic, or syphilitic. Of the first and last forms I have seen no examples. The rheumatic has been fully treated of by Dr Fuller, who, however, names so many remedies as to lead to a doubt which is the right one. It is the form seen in persons long subject to rheumatic pains in various parts of the body. A collier, who supplemented his earnings more legitimately made, by lying behind dykes and under hedges, and having a shot at any chance hare or rabbit which might come his way, came under my care a few years ago with a severe attack of sciatica. I have no notes of his case, but I remember that after every remedy I could think of—turpentine included—was tried ineffectually, he went into the workhouse, and came out crippled with his prolonged illness, but with his pain relieved by almost parboiling him in a hot bath. This group includes some of the very worst cases, which tax their attendants' patience and resources to the utmost. My object in this paper has been to point out the variety of sciatica which has appeared to me to be almost invariably benefited by oil of turpentine; and I now respectfully offer the results of my experience to the kind consideration of this Society.

## ARTICLE VI.—*On the Influence of Modern Education on Health.*

By JOHN M. STRACHAN, M.D., Dollar.

*(Read before the Annual Meeting of the Scottish Midland and Western Medical Association.)*

I GLADLY take advantage of this opportunity to bring under the notice of this Association a subject of great and increasing importance to the public, and of special interest to the medical profession; but which is entirely ignored by the former, and viewed with strange apathy by the latter. I refer to the relation which exists between school education and certain forms of disease amongst young persons.

Every medical man must have had frequent occasion to remark how closely diligence and success at school are associated with delicacy of constitution and frequent attacks of headache, stomach derangement, and nervous diseases; and most of us have had melancholy experience of cases where these lesser ailments have terminated in serious and fatal diseases connected with the head.

In Dollar, in consequence of its flourishing Academy, there are a great many families and a still greater number of boarders, who come from all parts of the world for the sake of education. I have thus, in my practice, had more than the usual share of patients among children attending school.

Many years ago, I was surprised at the frequency of complaints, which I had then no hesitation in ascribing to worms. The symptoms were headache, more or less severe, increasing towards evening, lassitude, loss or variability of appetite, irritability of temper, and want of sleep. When these symptoms were neglected, they not unfrequently terminated in a tedious fever of a low type.

I have said that I had no hesitation in ascribing these symptoms to worms; and I generally prescribed the usual anthelmintic remedies, which in those days (I speak of thirty years ago) consisted of a course of drastic purgatives, calomel and scammony, gamboge, turpentine or other severe remedies, the action of which kept the children from school for at least a fortnight. This treatment was always successful.

I shall not waste your time by detailing the steps by which I came to the conclusion, that worms had little or nothing to do with the evils produced; but that, in a great proportion of the cases, the cause of the symptoms was irritation of the brain, by too great or inappropriate mental work.

Whilst my change of opinion and treatment has been beneficial to my little patients, I cannot say that it has been conducive to my own ease or reputation. Formerly I had no difficulty in inducing parents to follow the active purgative treatment, even although the necessary consequence was absence from school. But now when absence from school, or diminution of lessons, is the only or principal remedy recommended, I usually meet with the strongest opposition, and all my physiological eloquence often fails to obtain a willing obedience. So that, if it were not for my abhorrence of quackery, I would be tempted to return to my old treatment, which, although unsound in theory, was effectual and far more easily carried out.

I have for many years had my attention directed to this subject, and I am convinced that the symptoms I have described, and many, far more serious evils, are produced by overworking the tender brains of children; and when we thoughtfully consider the amount of that overwork in our modern education, it is surprising that the evil consequences are not much greater than they have been; these consequences being prevented only by the powerful instinct for play that nature has kindly implanted in the young, which renders it impossible for many children to do what parents and teachers desire.

Children from five to seven years of age are generally confined in school for five hours, and those from seven to twelve or thirteen for six hours each day; during which they are engaged in very close mental work, or, at least, are expected to be so. And at most schools they have one or sometimes two hours of hard work, preparing lessons in the evening. That is to say, seven or eight hours each day of close mental labour, and that continued day after day. I am sure that few adult brains could bear this without injury.

I am aware that, although this work is imposed on thousands of children, comparatively few suffer as we would expect them to do. The reason of this, as I have already hinted, is, that, in the great bulk of children, nature is too strong for the schoolmaster, and a large portion of the time that is supposed to be spent in work is really spent in surreptitious play.

But, unfortunately, a considerable number attempt to do what is required of them. These are the precocious, the clever, or very good children, who from love of study, or from a sense of duty, or from the stimulus of emulation, overcome the instinctive craving for play. And it is they who suffer and fall into the doctor's hands.

It is a common mistake to suppose that the evils arising from overwork at school, are entirely owing to the want of muscular exercise and fresh air preventing the due development of the body; and there is a prevalent idea that the amount of work the brain does, however great, must correspondingly strengthen the mind. It seems to be unknown, or forgotten, that the brain is subject to the same laws as the other bodily organs, and, consequently, that whilst a due amount of exercise of any or all its organs will help to develop and strengthen them, a greater amount of work than is suited to their present condition will only weaken and perhaps seriously and permanently injure them.

Besides the over-amount of work imposed on children, much evil is done by the unsuitability of the kind of work to the age of the child. The different mental faculties are not all developed at the same time. The perceptive and remembering faculties are in active operation at a very early age; but the thinking faculties and the power of application are of much later growth. So we find that a very young child can easily be taught its lessons by oral instruction, but can learn by itself with the greatest difficulty; notwithstanding this, the practice is increasing for the master to teach little, but to require even young children to learn their lessons at home, to the great distress of the child, and I believe often to its mental injury.

The evils produced by overwork of the brains of children are not confined to the comparatively trifling ailments I have mentioned; I have already alluded to a low nervous fever which I have frequently seen following the neglect of the early symptoms of brain irritation. Chorea has sometimes a similar origin; and I have had fatal cases of acute hydrocephalus, which had been preceded for months by the symptoms I have described. If the history of precocious children and diligent scholars were pursued into their after life, I have reason to believe that many of them would be found with permanently weakened or diseased minds.

I have lately had two painful cases under my observation of young men, who, when at school, had been remarkably diligent and clever, and had carried off the dux medal and many prizes at school and college. One died in a lunatic asylum at about thirty years of age; the other, at the same age, died of general paralysis, a total wreck mentally and bodily. From my knowledge of their whole history as scholars, students, and clergymen, I have not the slightest doubt that the seeds of their disease had been sown at school.

I have no doubt that cases similar to those I have mentioned have not been unfrequent in the practice of those I am now addressing; our past experience, however, has been of a time when the inducements of teachers to overtask the tender and undeveloped brains committed to their care were trifling compared to what they now are.

Formerly the efforts of teachers were stimulated by a sense of duty, by the urgency of parents for the advancement of their children, and by the desire of popularity. But now we have the united efforts of the Legislature, the Privy Council, School Inspectors and School Boards,—all brought to bear upon the teacher to compel him to task the brains of children to the utmost possible extent.

By the system of payment by results, the worldly interest of the teacher is made to depend entirely on his doing this effectually. The pecuniary interests of the School Board also depend on the successful cramming of the children. What chance has the poor child against such an array of taskmasters?

There is a parish school in my neighbourhood, the teacher of which has long been eminent for making excellent scholars. Under the old regime, when the younger children had learned and repeated their lessons, if the weather were fine, they were sent out to the open air to play whilst the teacher was engaged with the older scholars; thus, a considerable portion of the six hours of school-time was spent in healthful recreation, by which their bodies were developed and kept in health, and their minds were relieved from continued strain. The progress of the children showed that their tender brains had sufficient work for their development and growth. But now, under the regime of revised codes, school inspectors, and school boards, in order to bring up the children to the standard required to get payment when they attain to seven years, it is thought necessary to give up the practice of sending them out to play; so an additional room has been provided for the purpose, and pupil-teachers employed to drill the little children during the whole six hours. Another teacher of my acquaintance not only thinks it necessary to drill the little ones with pupil-teachers, but also to add another hour to school time. As a natural result of all this, I find that the children who formerly spent their evenings in amusement now go home with their nervous systems so exhausted that they have no heart for play, and generally spend their evenings with their books, probably very much to the gratification of their parents and teachers.

This overwork is not confined to the very young. There is a standard for every age, in which, unless the scholar "passes," the teacher or school board receives no payment from Government; thus giving a strong pecuniary inducement for the teacher to strain the brains of the scholars to the utmost possible extent.

The standards may not be too high for the average capacity of children; but the very important physiological fact is unknown or forgotten, that the various mental faculties are not developed in all children at the same age. Just as some children cut their teeth, or are able to walk earlier or later than others, so the various mental faculties are much later of being developed in some children than in others. There are thus a number by whom the average attainment cannot be acquired; and the attempt to force the undeveloped organs is productive of great mischief.

Let us suppose that all these efforts were made to get the greatest possible amount of bodily labour from young children, what would be the result? This was shown forty years ago by the Report of a Parliamentary Commission, which so shocked the public by its description of enfeebled, stunted, and deformed bodies which were the result of overworking children, that the Legislature was compelled to pass the Factory Acts, which are now in operation for limiting the hours of employment of children and young persons.

I need not say to this meeting that the weak, tender, undeveloped brains of children are far more likely to be severely and permanently injured by overwork than their bodies; but, unfortunately, the public—even the intelligent and educated public—are entirely ignorant of this. It is not unknown that continued, and especially forced and unpleasant, brain work is destructive to the health of adults; but it seems to be taken for granted that no amount of brain work can be too great for children.

The public is entirely ignorant of the physiology of the brain, and consequently of the effects of the present system, which will probably not be fully known till the results are seen many years hence in enfeebled, stunted, and deformed minds.

But medical men are not thus ignorant, and to them, with their knowledge of the nature and capabilities of the brain in early life, it must be evident that the attempted overworking of the weak and undeveloped brains of children must be destructive to the mental and bodily health of the rising generation.

I have no doubt that this meeting will agree with me that it is our duty to raise a warning voice on this subject. But it is very difficult to know how this can be done with effect. Individual efforts can be of little avail. Our influence is only over parents, but the fault does not rest with them, who, generally speaking, could be easily persuaded not to oppress their children with overwork. Parents have little or no power over teachers, and, indeed, teachers have now little or no power over themselves, but must comply with the rules imposed upon them.

It is evident that the main design of the Code prepared by the Privy Council is to get the greatest possible amount of work from the teacher; and no thought has been given to the influence of these rules on the health of the children. The only explanation of this is, that Privy Councillors, School Inspectors, Teachers, and the educated public generally, are all alike ignorant of even the elements of physiology, and I fear that whilst this is the case, there will be no way of drawing general attention to the subject.

It might have been expected that, on a subject so strongly bearing on the public health, advice might have been sought from the medical profession. But it is not less strange that the profession has not long ere now made its voice heard on the subject.

Some warning voices have been raised. Forty years ago an excellent work was published in America by Dr Brigham, and has been frequently reprinted in this country, in which the evil effects of too great mental work, both in old and young, were clearly and forcibly pointed out. Combe, in his excellent physiological works, dwells on the same subject; more recently, in Clarke on Sex in relation to Education, in Dr Maudsley's and Dr Carpenter's works, the evils of our modern education are denounced. But all these appeals fall powerless from the want of some knowledge of physiology on the part of the public.

I hope that the efforts to introduce into schools instruction in the elements of physiology may ultimately have a good effect. But, in the meantime, ought not teachers to be instructed in the nature, capabilities, and order of development of the mental organs which they undertake to mould? I do not know what are to be the duties of the new professors of education; but I earnestly hope that to give instruction in mental physiology to our future instructors will be an important part of their functions.

In concluding this address, I have to express my regret that I have no suggestion to make as to what is the duty of medical men under the present circumstances; but I beg earnestly to recommend the subject as one well worthy of consideration and discussion.

## ARTICLE VII.—*Case of Tetanus occurring after a Burn.*

By J. B. CARRUTHERS, M.D., etc.

THE most effectual remedy for the successful treatment of tetanus is yet an unsettled question in medical science. The pathology of the disease, which is still very obscure, and may still be considered as undetermined, has afforded little or no assistance to the medical practitioner; while the appearances presented on post-mortem examination, though they have thrown some light upon the nature of the disease, have as yet been of little or no service in aiding him in determining the best course to pursue in the treatment of this disorder. The treatment, therefore, hitherto resorted to, has been, for the most part, of a very vague and unsatisfactory description; many remedies of directly opposite qualities have often been prescribed, and occasionally with success. In administering remedies, their action should always be carefully observed; and if beneficial, persevered in and recorded. I therefore deem it proper to bring under the notice of the profession a case of tetanus supervening a burn, which recently occurred in my practice, and which got well under treatment. Having been for some time convinced of the therapeutic value of chloral in combating the involuntary movements of chorea, I successfully prescribed it along with bromide of patassium in a case of tetanus supervening personal injuries, which occurred in a boy æt. 14. (*Vide Lancet*, 1874.) I applied the same medicines, with a like good result, in this peculiar case, for I believe there are very few cases indeed, if any, which are on record, where tetanus supervening a burn has got well under treatment. In this instance the patient, G. F., a boy aged 10 years, who had enjoyed fair average health hitherto, fell into a large boiler full of boiling turnips on 9th December, scalding both his legs from the knees downwards, the calves of his legs being most severely injured. I got his legs thoroughly cleansed of the mashed turnips, and applied cotton wadding to his legs after dusting them over with flour; and as they were very painful, I gave him 10 drops of vinum opii. During the first twelve days the boy progressed favourably, taking his food, and when restless took his drops; but on the evening of 22d Dec. he was seized with a rigor, and complained of pain from his head down the entire length of his spine. I then felt his cheeks over the masseters, and found those muscles rigid; and on trying to open his mouth, I could only open it a very little; his expression was anxious and livid, and his forehead was covered with profuse perspiration. I applied a belladonna plaster over the entire length of his spine, and ordered him 20 grains of bromide of potassium, and 10 of chloral hydrate in syrup and water, every three hours. On taking off the cotton wool there was a large quantity of fœtid, sloughy matter on the calves of both legs. I dressed them for the night with charcoal poultices, made with water in which was put a glass of Condy's fluid. His pulse was 125, temperature 103·2°.

Next day, *23d Dec.*, the boy had slept about two hours during the night; pulse 120, temperature in the axilla 103·1°. The legs looked healthier, but were more sensitive. The medicine continued, as the boy had several tetanic spasms in the morning.

*24th Dec.*—The legs were very red and irritable, and I ordered the poultices to be stopped; and dressed the sores with a lotion of Condy's fluid half an ounce, and chloral one drachm in a pint of water; pulse and temperature the same as yesterday. His mouth would scarcely admit the tube of the feeding-cup by which he was fed on milk — his only nourishment during this attack. While feeding him in this manner, a series of spasms came on, with severe wrinkling of his brows; when these passed off he lay down exhausted.

*25th Dec.*—Slept well during the night. At the time of my visit he took a good drink of milk; when finished he was seized with a rigidity of the muscles of the left side from the neck to the knee; pulse 112, temperature 102·3°. The sores improving, a slough having come away from the calf of the left leg.

*26th Dec.*—Passed a favourable night. Complained to-day of stiffness in his throat, crying out that some one was choking him from the head of his bed; pulse 105, temperature 101·3°. In the evening an enema of castor-oil and water was given. The action of the chloral was now seen in the congestion of his eyes, which were evidently in great excitement and distress.

*27th Dec.*—Slept for six hours consecutively during the night, and on awakening his bowels opened satisfactorily from the enema; and as his muscles were now much relaxed, and as he was much excited, I stopped the chloral, but continued the bromide for other two days in 10-grain doses; pulse 98, temperature 100·1.°

*28th Dec.*—Much improved; has slight spasms, which only last for a minute or two. From this date, on the suggestion of Dr Joseph Bell, I dressed the legs with oxide of zinc ointment; pulse 86, temperature 99·1°.

*29th Dec.*, 10 A.M.—Newly awake; was very talkative. In answer to my question he said he was fine, but would not believe he was in his own home; and on reasoning with him, he clutched my hand with such force that I could scarcely disengage it.

*30th Dec.*—Progressing favourably. Pulse 86, temperature 98·2°. The legs healing satisfactorily, and the spasms abating, I ordered the bromide to be discontinued.

*31st Dec.*—Had a good night's sleep, only was excitable, and would not believe where he was; but was partly satisfied when he got a drink of sweet-milk from his own cup. From this date onwards my little patient gradually recovered, although for some time the peculiar action of the medicines was manifested in a remarkable degree, both in originating and exciting false ideas in his imagination. It is quite impossible to convey an idea by description of the extraordinary manner in which he conducted himself, both in seeing things in the room that were not there, and in engaging in battles with imaginary foes; gradually, however, all these illusions and hallucinations lessened, and ultimately left him. It is perhaps worthy of being recorded that the administration of the drugs was begun on the first manifestation of the disease, and freely continued for five days, till he had taken 400 grains of chloral and 800 of bromide; and when the physiological action of the medicines originated, they seemed to act therapeutically by relieving the spasms of the muscles. It appears from this and my previous case, as recorded above, that these medicines have a beneficial effect in relieving tetanic spasms only when they produce these delusions and hallucinations; the muscles relaxing most when the physiological action of the medicine is established. When we consider the great nervous depression that invariably occurs after an extensive burn, coupled, as it was in this case, with the tetanic seizure, it appears to be worthy of being recorded, for there is no class of diseases so replete with physiological and therapeutical interest as diseases of the nervous system. Every case is comparatively an experiment, with this difference, that in an experiment we know the cause and observe the effects—in a patient with tetanus, we see the effects only, and have thence to infer the cause and look out for a remedy, which has been done here.

It is expected in a short time that there will be no trace of the accident, except the marks on the legs.

## ARTICLE VIII.—*On the Etiology and History of Leprosy.*

By W. MUNRO, M.D., London, late of Cupar-Fife.

(*Continued from page* 440.)

FROM the time that leprosy fairly gained a footing in Europe, it can be clearly traced in its onward course. The words of Celsus and Lucretius, already quoted, and the positive testimony of Pliny[1] to the effect that it was unknown in Italy until *the return of Pompey's soldiers from the East* (B.C. 62), show that it reached Italy in the first century before Christ.[2] Galen,[3] writing about 180 A.C., speaks as follows (after speaking of its existence in Alexandria): "At in Germania et Mysia rarissima hæc passio videtur, et apud Scythias lactes potatares, nunquam fere apparuit;" showing that it had spread beyond Italy in his time, although it was still very rare in Germany; but four centuries after this, from the researches of Virchow,[4] we learn that it had already spread so much, that, in 636, leper houses were already established in Italy, Verdun, and Mæstricht. In 757 Pepin of France, and in 789 Charlemagne, made marriage of lepers illegal, and leprosy a sufficient cause for divorce.[5] In the ninth and tenth centuries, leper houses were probably established in Bremen and Constance. Thus, by the end of the tenth century, the disease had already spread to some extent all over France and Germany.

Now let us glance at some of the great historical events which had taken place in Europe during the period of fully ten centuries during which we have just traced the progress of leprosy. Just before, and at the time of Christ, Rome had become the mistress of the civilized world, and communication between all parts of the empire was, for those days, easy and constant; armies were constantly leaving and returning to it, *bringing captives from all parts.* Jerusalem had fallen, and the Jews were scattered. All these facts may be kept in view when we read the words of Galen, already quoted, and show, along with what we are told by Pliny, that Rome, in gaining an empire, paid as one penalty the infliction on generations then unborn of the most loathsome scourge, the most hideous death in life that has ever afflicted the human race. This scourge was not only carried to Rome by her returning armies, as Pliny states, and as we know also it was to France in modern times from Egypt,[6] but also without doubt by those of the numerous captives taken to Rome. From Rome, again, it was carried by the constant communication going on between it and all parts of the empire, including France, Germany, and Spain,[1] up to the time of the fall. In the fifth and sixth centuries the disease would be still further spread by the conquests of Alaric and others, and the return of *their* armies with captives to their native places.

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[1] Pliny, lib. xxvi. c. i. of the Historia Mundi; and p. 154 of vol. v. of Bohn's edition.

[2] See also Hirsch, p. 305; and Hebra, vol. iv. p. 121.

[3] De Arte Cur. lib. ii. cx.

[4] Granulations geschichte (p. 506), which I thank him for sending me; also in Archiv, B. xviii. and B. xx.

[5] Lepers were also looked upon as dead.

[6] Larrey, Relation Histor. et Chir. de l'Armée en Orient, 1803, p. 236. Only

We are informed that leprosy was first carried into Spain by the Roman troops soon after its outbreak in Italy,[2] and that it was common there in the tenth century. This, I may point out, is of importance in connexion with its spread in France, where it appears to have somewhat increased in intensity about the time of Pepin, as he in 757 promulgated the law already mentioned, this being *about forty years after France had been invaded by the Saracens from Spain,* in which country leprosy was increased after their invasion of it;[3] an increase, be it remarked, noticed about two centuries after that invasion. Doubtless the frequent invasions by the Lombards in the sixth century, they having the disease among them, would also spread it in France.

From the late Sir James Simpson's classical papers on the subject,[4] we learn that leprosy had reached Wales in 950, as Noel Dha, then king, passed a similar law to that of Pepin in that year.

We now come to a period in the history of leprosy when all authors who have investigated the matter agree, however differently their opinions may have led them to explain the reason, that there appeared to be a great increase in leprosy all over Europe,[5] and when it first really seems to have become very severe in England—I mean the period of the Crusades. Hirsch does not certainly consider this absolutely proved, thinking that the apparent increase may have been only from a more scientific spirit in the medical world of the time, and an increase in the number of leper hospitals at the same time, partly no doubt as a consequence of that spirit;[6] but surely this is less likely than that the increased

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a few cases returned, as Napoleon's occupation of Egypt was short. This chiefly, and also the improved condition of the French as compared with former times, may have been the reason of its not spreading. Nor, be it remembered, were any Egyptian captives taken to France.

[1] Marejon, quoted by Hirsch, p. 305.

[2] Ozanam, quoted by Hirsch, p. 305.

[3] Hirsch, p. 305.

[4] Ed. Med. and Surg. Journal, 1841, vol. p. 301. He also proves clearly in these papers that the lepra of the middle ages, the English leprosy of Gilbert (1270), and the Elephantiasis Græcorum of Bateman, etc., were identical (vol. i. 1842, p. 135 to 140). And it is pointed out as clearly by Dr Charles Macnamara (Leprosy, Calcutta, 1866) that the leprosy of England of the middle ages described by Gordon, was identical with the "Kushta" or leprosy of India, as seen there at the present day.

[5] Dan. et Boeck, lib. cit., p. 182; Simpson, lib. cit., 1842, p. 395; Hebra, vol. iv. p. 122; Michaud, History of Crusades, vol. ii. p. 308, etc.

[6] Lib. cit., p. 306.

number of lepers then required more leper houses, as their decreasing number from the fifteenth century onwards required fewer, especially as Hirsch expressly quotes Mezeray (Histoire de France, vol. ii. p. 168), to the effect that all the towns and villages in France in the twelfth century had leper houses. He also quotes Muratori (Antiq. Ital. Medævi, iii. 53) to the same effect as regarded Italy.[1] He also as expressly states that many who went away healthy (der gesund ausgog) returned from the East afflicted with leprosy. His idea that many of the cases of so-called leprosy were really cases of syphilis is no doubt correct, but it does not seem to have occurred to him that such cases would be *less* likely to be confounded with leprosy after the uprising of the scientific spirit he speaks of than before, and so many the less lepers would there have appeared to be as compared with previous centuries, had there not been a real and absolute increase in their numbers. In conclusion, I may say that Hirsch speaks of leprosy as being between the twelfth and fifteenth centuries "one of the widest spread (verbreiteten) chronic constitutional diseases, playing a no less important rôle than syphilis has in our time,"—an admission hardly compatible with his own scepticism as to its increase at that time. That it was widespread there can be no doubt, as, according to the testament of Louis VIII. (1229), quoted by Michaud, there were at his time 2000 leper houses in France alone, and it has been stated 19,000 throughout civilized Europe.

In England, the first leper hospitals established seem to have been that of St Giles, London, in 1101,[2] and York about 1110; and from that time to 1472, a hundred and twelve such hospitals, all richly endowed, were built in England. Leprosy reached Scotland before 1177, three-quarters of a century after the first leper hospital was built in England; and many such hospitals were built over the country, at Aldcambus, Gorbals of Glasgow (1350),[3] Greenside, near (now in) Edinburgh (1589), etc., and in Elgin in 1226.[4] The latter date is to be noted as being at least forty years before the establishment of a leper hospital at Bergen in Norway, the part of that country in which leprosy has always been most severe, and is so at the present day,[5] it being, as Simpson points out, the *nearest* to Scotland,—a point of no little importance.

It also appears to have spread slowly northwards to the Shetlands, Faroë Isles, Iceland, and Greenland. The last part of the British Islands to be reached was, I must here point out, the one to which, looking on its introduction as only possible through more or less prolonged intercourse of the sick with the healthy, and at the extremely rare communication with any other part of the kingdom, we should have actually expected it would reach last—I mean the remote island of St Kilda, which it only reached about 1680,[1] after it had died out in England, and when it was on the decline in Scotland, at least in the southern counties where records remain of it.[2]

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[1] Lib. cit., p. 307. Mezeray's words are—"Il y avait ni ville, ni bourgade, qui ne fust obligée de batir un hospital pour les (sil. lepreux) retirer;" and Muratori—"In Italiam vix ulla erat civitas, quæ non aliquem locum leprosis destinatum, haberet." Could anything be stronger, even if some cases of secondary syphilis were confounded with leprosy, as they doubtless were!

[2] Simpson, 1842, p. 428. [3] Ib. 1841, p. 329. [4] Ib. 1842, p. 148.

[5] See Carter's Rep. on Lep. in Norway, map, etc.

It also spread to Holland and Denmark, as well as Sweden and Ireland.

We must now slightly turn back to trace it into Russia, the only country in Europe where it has never been universally prevalent, *except* (and this is a most important exception) in those parts which have been at all times within the historical period the highways of commerce throughout that country, viz., the southern provinces, and more especially the Crimea and the provinces lying to the east of it.[3] It has also been observed in Finland, Esthonia, and Courland; but its presence in them is only another proof of its communicability, they being rather Swedish and Polish provinces than Russian, only having become part of the latter empire since the time of Peter the Great. In the great interior of Russia leprosy is unknown,[4] and that is precisely the part of the empire which, up to three centuries ago, was practically cut off from direct communication with the outer world. Voltaire[5] does, indeed, repeat a fable of Pliny's of some *Indians* being wrecked on the Elbe, and tries to give colour to it by saying that they might have come through Persia to the Caspian, and thence by the Volga and Perm to the Baltic, and on this less than slender foundation seems to argue that there was some early commerce to these regions; but as the Elbe is 1700 miles from Perm, and as the ideas of tribes and nations outside of the Roman empire entertained by the Romans at the time of Augustus were of the most confused kind, I cannot see that there is any reason either to believe the fable, or Voltaire's deduction from it. It may be true that Perm, on the extreme east of European Russia, in 60° N. lat., was, at the time of the Caliphs, a depot for the furs of what is now North Russia, where they were exchanged for the goods of Persia; but such commerce would not by any means bring the *people* of these two remote countries into contact, except in the case of a few traders of each country who would meet at Perm, and could have almost no effect in the transmission of a purely contagious (if not simply an innoculable disease, as I am inclined to think leprosy is), although infectious diseases might be so carried. But it is remarkable that leprosy is said to have been observed in Kasan;[1] and if such were the case, it would be an instance of the disease going just as far as actual personal commerce with the south was carried on, and no farther. Kasan is traversed by the Volga, giving free means of commerce from the south. It and Perm were both included in the "Tatar" empire of Gengis Khan and his successors.

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[1] Simpson, lib. cit., 1842, p. 141.

[2] Even yet St Kilda is in one sense more remote from Scotland than Australia. Letters may be months delayed going and coming, as appears from complaints in the *Scotsman* lately. I wrote to the minister fully five months ago, asking whether any cases of leprosy still existed, but no answer has reached me yet. Dr Latham of Cambridge visited the island last year, and (as he kindly informed me by letter) saw no cases of the kind. He was known to be a doctor, so any sufferer might have been shown to him; but it is just possible that such cases might have been kept out of sight.

[3] Hirsch, p. 315. [4] Ibid., p. 316. [5] Hist. de Pierre le Grand, p. 35.

I have thus spoken at some length to point out strongly the absence of leprosy in the centre of Russia, and also in Lapland and the other northern provinces;[2] these being the very provinces having no contact with infected countries, as I believe that it is from such proofs that the communicability of the disease can be made clear.

The date of its introduction into Russia is unknown. Hirsch (p. 314), quoting Krebel, considers it likely that it was already endemic in the 15th century; and Richter[3] says that it only reached Russia in 1426. But this last author is decidedly wrong, as leprosy is expressly mentioned by Rubruquis as having been seen by him in the Crimea in 1236.[4] As true leprosy is now very common in the Crimea, there is no reason for doubting that it was the same disease.

As additional proofs of the connexion between the spread of leprosy and communication with infected countries, the following historical events may be kept in mind:—1. The Crimea was a Greek colony, founded about 550 B.C., and communication was naturally kept up for centuries afterwards with the mother country. 2. In the middle ages it was the country through which all commerce passed from east to west, and that commerce passed along the Volga and through the south-eastern provinces of Russia, where leprosy is most common to this day.[5] It is just possible that leprosy may have been introduced into Southern Russia at a very remote period of history by either of the following events,—(1.) Rameses or Sesostris, king of Egypt, penetrated as far as Scythia about 1350 B.C., and left a colony of soldiers behind him at Colchis.[6] (2.) The Scythians under Madyes, as the Ogus Khan of the Tartars, penetrated as far as Egypt, and remained in Persia twenty-eight years before they were driven back, 610 B.C.[1] (3.) Darius left a large part of his army in Scythia about 510 B.C., those left being chiefly the sick and their guards. Under the circumstances they probably fraternized with the Scythians.[2]

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[1] Hirsch, p. 316, Note. Blosfield says that cases occur, *arising* from syphilis. Hirsch says there are doubts as to the correctness of this idea.

[2] Voltaire says of the Lapps that they were "sans maladies."

[3] See Virchow in Coll. of. Phys. Rep., p. 1867 (quoted from Richter's History of Med. in Russia, vol. i. p. 245). Virchow says that it is only highly improbable that Richter's idea is correct.

[4] See Travels of Rubruquis, in Kerr's Collection, vol. i. p. 183.

[5] Rubruquis, p. 196; and Marco Polo, 1260-95, in Kerr's Collection, vol. i. p. 272.

[6] Herodotus, lib. ii. 102 *et seq.*; and Lepsius, quoted in Humboldt's Cosmos, vol. ii. p. 487.

Thus there has been abundant communication between the south of Russia (and more especially the south-east) with countries infected with leprosy, and the consequence is seen in its existence there; while, as already stated, it is unknown in the parts which have had no such communication.

I may finish what I have to say about Russia by saying that the disease seems to be unknown in Siberia, with the exception perhaps of Kamtschatka;[3] in which, however, syphilis is exceedingly prevalent, so that Inozenzoff, the author whom Hirsch quotes, may, as Hirsch says, have been mistaken. Kennan also indicates the frequency of syphilis; and as neither Cook, or King, who not only visited, but also travelled in the country (although the latter mentions a number of diseases he saw[4]), nor Beechy (1825), nor Kennan, who lived for some time in Kamtschatka (1868), mention leprosy, it is probably a mistake. As the Kamtschatdales, however, were decimated by smallpox in 1767,[5] and the Empress Elizabeth compelled a great number of Cossacks to immigrate into Kamtschatka, it is possible that they may have taken it with them from their homes in the south of Russia. The same doubt exists as to its existence in the Aleutian Isles, where Dr Anderson, Cook's surgeon, makes no mention of having seen it, though he mentions cancer and syphilis.[6]

Let us now return to Western Europe. The decline of leprosy as a great epidemic, lasting for centuries, took place very much in the order in which it attacked each country; although countries *in which the condition of the people improved most rapidly, in which the strictest segregation and the most severe laws against leprosy existed*, got rid of it at a comparatively earlier period than those labouring under the reverse conditions; a marked instance of which is Norway, in which it lingers to the present day, the country being still poor, and only lately having passed laws encouraging segregation, and never having had severe laws affecting lepers.[7] Thus, in England it was disappearing by the end of the fourteenth century, and appears to have entirely disappeared about the early part of the sixteenth;[8] while it still existed, though rare, in Italy (where it began so many centuries earlier than in England) at the end of the fourteenth century,[1] but still lingered until the sixteenth century, and even thereafter remained in a few spots: it only exists in one spot now.[2] In France it was still common at the end of the sixteenth century,[3] though *tubercular* leprosy may not have been so common, and it had not entirely died out in 1789: a very few cases even exist in some parts of the south coast at the present day.[4]

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[1] Universal History, vol. xx. p. 31; and Herodotus, b. iii. c. 103.

[2] Herodotus, b. iv. 134. His whole army consisted of 700,000 men, so a great number must have been abandoned.

[3] Hirsch, p. 314, and Chapter on Syphilis, p. 358.

[4] King's Continuation of Cook's Journal (Voyages), vol. vi. p. 158. Second Voyage, 1772-74.

[5] King's Continuation of Cook's Journal, vol. vi. p. 327.

[6] Vol. vi. p. 390, *et seq.*; and pp. 470 and 480 of Cook's Voyages.

[7] See Daniellssen and Boeck, p. 184.

[8] Liveing, p. 25 *et seq.*

In Germany it was still common in the middle of the sixteenth century, though dying out. In the Netherlands it died out in the middle of the seventeenth.[5] In Denmark it was gone in the middle of the sixteenth century. In Sweden a leper hospital was established as late as 1631, and many cases still remained at the beginning of the present century.[6] In Norway, as already stated, it still exists, though chiefly confined to the district of Bergen.

In Spain it almost disappeared later than in France, but it still remains in a few places up to the present century.[7] It still exists in Portugal.[8]

Returning to our own country, we find that it remained longer in Scotland than in England, only being stamped out in the former country about the end of the sixteenth century, but effectually when it was so.[9] It lingered on in the Shetland Islands till about 1742, and one case was seen in 1798, in a man in whom it appeared to be hereditary.[10]

It disappeared about the end of last century from the Faroë Isles.[11] It was seen in Iceland by Robert Chambers[12] about seventy miles inland in 1855, but appears, from what Burton says, to have become extinct now.[13]

Thus we see that England was one of the first countries from which it disappeared, it being a country in which lepers were strictly kept apart from the healthy. They were driven out of London in 1346.[14] At the same time, from the time of the Norman Conquest, it was one settled kingdom in which no very long-continued intestine wars, with their direct consequences, famine and pestilence, ravaged the country. As a constitutional State, it encouraged by its laws internal improvements among the people, which were impossible in the down-trodden petty States of continental Europe, with their splendid kings and dukes and starving peasantry. Compared with France, which up to 1355 was a mere jumble of petty

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[1] Beneveni (quoted by Hirsch, p. 308) and Dan. and Boeck, p. 182, says, in speaking of a case seen elsewhere, "morbus qui in Italia pene numquam visus."

[2] Hirsch, p. 308.

[3] Ibid., p. 308.

[4] Dan. and Boeck, p. 185, and Hirsch, p. 318.

[5] Hirsch, p. 309.

[6] Dan. and Boeck, p. 182.

[7] Hirsch, p. 318.

[8] See Peacock in Lancet, vol. ii., 1870, p. 775, and Hirsch.

[9] Simpson, lib. cit., p. 325.

[10] Simpson, lib. cit., and Edmonstone's Ancient and Present State of the Zetland Isles, vol. ii. p. 102.

[11] Liveing, quoting Hjort, p. 31.

[12] Tracings of Iceland and the Faroë Isles, Chambers' Journal, Oct. 1855, p. 261.

[13] A Summer in Iceland, 1875, vol. i. p. 153.

[14] Simpson, 1842, p. 419.

States, in which for five hundred years "anarchy and ignorance"[1] prevailed, and in which famines were frequent, and the poverty of the people, as compared with England, was horrible to think of,[2] and in which many were really slaves up to the time of the great Revolution;[3] as also with Germany, where famines were in the middle ages by no means unfrequent.[4] England was far advanced in comforts and civilization, while they were still in a state of extreme wretchedness. No doubt, compared with the present day, the condition of the English people was poor enough, even the courtiers often eating bad food, as mentioned by Philip de Blois, and dearths being by no means unknown. I only wish to point out decidedly that the English, as a people, were better off, in the period under consideration, than Continental peoples, and rose much more rapidly above constant want; and I believe, as a direct consequence, leprosy disappeared earlier from among them. I may as well say here that I do not think it possible that simple want could ever *create* leprosy, but, as a secondary cause, it lays a population open to the attacks of the disease when the contagion is brought into a country; and, on the contrary, as I shall hereafter try to show, a flesh-fed population is protected from such contagion. Scotland, again, was always a much poorer country than England, and in a much more unsettled state. The English soldiers are said to have been astonished at the poverty of the Scottish camp after the battle of Pinkie (1547),[5] and oatmeal is still in a great measure the staple food of Scotch farm labourers (although by no means their exclusive food, as many English people think), just as it was two and more centuries ago in England.[6] This extreme poverty, combined with the disturbed state of the country up to and even after the Union, would tend in Scotland to counteract the good effects which, so far as stamping out the disease was concerned, were certainly calculated to be produced by the rigorous and barbarous laws enacted against lepers. At Greenside, near Edinburgh, they were not allowed to leave the hospital under pain of death.[7] A leper woman quick with child was buried alive.[8] The state of slavery[9] in which many of the people were steeped, with the abject poverty accompanying it, are also to be kept in mind. Such laws[1] would, no doubt, along with the general avoidance of lepers, assist in its total suppression; and we have to thank our forefathers for acting as they did, that we have not a remnant at least of the disease among us at the present day.

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[1] Hallam, Hist. of Middle Ages, p. 120.

[2] Ibid., p. 19 *et seq.*, and p. 125. [3] Ibid., p. 106. [4] Ibid., p. 103.

[5] Hugo Arnot, Hist. of Edinburgh, 1779, p. 55; yet it had improved from the fourteenth century. He states, p. 194, that Fletcher calculated 200,000 as the number of beggars in Scotland in the sixteenth century.

[6] Dodd, the Food of London, p. 77. The whole historical part of the work shows that from the fifteenth century, and even earlier, the English were a well-fed people on the whole, although partial local famines might occasionally take place from want of communication. Going further back, Matthew Paris is quoted in reference to a famine in which great numbers died in 1258 (p. 27). No doubt also much of the food eaten was salted, unless near the sea.

[7] Hugo Arnot, Hist. of Edinburgh, 1779, p. 194.

[8] Simpson, 1842, 418 *et seq.*; (Dan. and Boeck, p. 118, mistakenly quote this "burned" (brulé).

[9] Hugo Arnot, Hist. of Edinburgh, 1779, p. 52.

Going slightly back, and comparing the food of the Italians with that of the English, the latter will be found to have always been much superior, and, it may be remarked, included an abundance of one article which the Italians do not use—milk; yet of the value of which as an article of diet calculated to prevent the spread of leprosy even Galen seems to have had some idea, when he remarked on the absence of the disease among the Scythians—"lactis potatores," in a passage already quoted. Now, I have already shown that leprosy lasted a much longer time as an epidemic in Italy than England, where it seems only to have come in with the Saxons (though the first leper-house was built in 1101; at least the Saxons had words for it) at least five centuries after it began in Italy, yet it was as late, if not later, of dying out there, and did not do so entirely even then.

Spain, where leprosy still lingers, has never been a settled country.

I have seen it stated that the present yearly consumption of flesh meat in Spain is only about 24 lbs., as compared with over 180 lbs. in England.[2] Dodd (pp. 248-50) gives the London consumption of 1750 as 70 lbs. per head; in 1840 and 1852, 120 to 140 lbs.; that of all the great towns of England, 100 lbs.: all far above that of Spain, while the increase in the English consumption is noticeable.

Now, passing to Norway, we find that at the present day the food of the people is as poor as that of England was some centuries back. Fresh meat is never eaten, and the greater part of the nourishment is from oatmeal, potatoes, and sour-milk.[3] Besides this, segregation has only been carried out lately; during the last twenty years, the disease up to that period, "as in other countries, showing no natural tendency to subside,"[4] but now diminishing rapidly since proper asylums were erected.

In Sweden, where the food of the people is now abundant but coarse, and milk, butter, and cheese plentifully used, as I have myself observed, and where meat is more common than among the peasantry of other countries, though unfortunately it is chiefly used salted,[5] there being an aversion to fresh meat, the disease has died out during this century.

In both these countries the food has somewhat improved of late.

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[1] By an Act of the Scotch Parliament in 1427 all lepers were to be secluded (Geo. Munro in Preface to Public Health Act 1867, p. 1).

[2] This is from a newspaper cutting. I should be greatly obliged to any one who could refer me to the original calculations.

[3] Carter's Report, p. 31.

[4] Ibid., p. 28.

[5] Harper's New Monthly Magazine, Jan. 1871, "Folk Life in Sweden."

In the Shetland Islands, where it lingered on for three-quarters of a century after becoming extinct in Scotland, Edmonstone,[1] in 1809, tells us that "the lower class lived chiefly on bread, milk, and fish," the fish being dried, and putrefaction favoured, it being in that state esteemed a delicacy. From what I myself saw in Shetland fifteen years ago, the condition of the poorer classes is now much improved, fowls and meat being plentiful and cheap, and I never saw any used that was tainted.

I need hardly say that Iceland is essentially a poor country, famines not being unknown in the present century, and driving many to America.

As to Greenland, there is a want of late information on the subject.

Thus, I think I have shown that England, in which the improvement in the condition of the people was most rapid, while segregation was carried out at the same time, got rid of the disease most rapidly; while the opposite conditions prevailing to a greater or less extent in other countries, it prevailed accordingly a greater or less time in them.

We will now follow the disease to the Western Hemisphere, its introduction among the indigenous tribes of which is one of the strongest proofs of its communicability.

(*To be continued.*)

## ARTICLE IX.—*Treatment of Wounds of the Radial and Ulnar Arteries by Acupressure.* By J. P. BRAMWELL, M.D., L.R.C.S. Ed., one of the Visiting Surgeons of the Perth Infirmary.

THESE arteries, from their comparative superficiality, are often wounded, the most frequent causes being cuts with glass, as when the hand goes through a window-pane, and the slipping of a chisel off a piece of hard wood on to the wrist. My late preceptor, Sir Charles Bell, used to give very graphic descriptions of this injury, which in some cases, after a series of futile attempts to control the hæmorrhage, terminated ultimately in amputation of the arm itself.[2] It is to be hoped that such a remedy is now obsolete; but still, it must be confessed, that to tie the radial artery in such circumstances, especially when the veins are also wounded, the parts infiltrated with blood, and the light artificial, is not an easy matter. Much tedious dissection is often necessary, and the parts afterwards suppurate and heal slowly.

For some time past we have adopted a much simpler and perfectly efficient remedy, namely acupressure. This plan is easy of performance, and can be quickly carried out. It is a surgical maxim that in all such cases both the proximal and distal ends of the wounded artery must be tied; but it is a singular fact, and one which I communicated to Sir James Simpson a short time before his death, that I had been successful in two cases in arresting hæmorrhage from a wounded radial artery by one (proximal) needle alone. When this plan is adopted, the acupressure needle is thrust down close to the artery on its ulnar aspect, and its point brought out a considerable distance from the side of the vessel on its radial aspect. In this way the collateral vessels seem to be caught and strangled, so as to prevent the blood finding its way into the distal end of the artery.

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[1] Lib. cit.

[2] See Bell's Surgery, vol. i. p. 66.

Where the wound in the wrist is very free I prefer two needles, one proximal, the other distal; and each should be inserted close to the artery, and the point brought out close on the other side. The needles are then "kinched" with a strong thread, twisted round into a figure of eight, and drawn tight. A bandage may then be applied, or simply a cold-water rag. In four or five days the needles may be removed with safety, but if any fear of fresh hæmorrhage is entertained, the "kinching" thread may be cut and the needles left for twenty-four or forty-eight hours longer.

We would venture to recommend this mode of procedure as decidedly preferable to ligature in wounds of the radial or ulnar arteries.

# Part Second.

## REVIEWS.

*Smellie's Treatise on the Theory and Practice of Midwifery.* Edited, with Annotations, by ALFRED H. M'CLINTOCK, M.D., etc., etc. The New Sydenham Society. London: 1876.

THIS book begins with a fine engraving of the author, in what we may call the old style, suitable to the subject. It is creditable to Dr M'Clintock's taste, and to the skill of Mr Anderson, the artist. In every part of the world medical men will now, thanks to these gentlemen, for the first time look on the face of an old London Scotch obstetrician, whose memory everywhere is cherished, and whose name commands the highest respect. The picture is one of peculiar and rare interest, for although we have always had and have now distinguished artists in our ranks, we do not remember of portrait-painters among them; and, in his delightful biography of Smellie, M'Clintock tells us that this portrait, now in the possession of the Edinburgh Royal College of Surgeons, is probably by Smellie himself. We refer to the biography for satisfactory evidence to this effect. Had the New Sydenham Society done for Smellie's memory no more than the publication of this valuable print, it would have a strong claim on the gratitude of the profession.

M'Clintock's life of Smellie is a very interesting contribution to medical literature. His works, of which we shall have a few words to say, show that he was a very great man and midwife; but his biography was needed to show his peculiarities. Of these we shall mention some; and the most striking is his writing and publication of his works chiefly after his retirement from practice. No doubt, like many other Caledonians, he early recognised and acted on the true saying, that one of the finest views in Scotland is the road to England. But he did not go south for mere pelf. His whole career testifies that love of filthy lucre was not his guiding principle, that he did not write in order to effloresce in public prints and advertisements, but that love of his fellow-men and of science—a love which survived his love of money and lasted till his death—was deeper in his bosom than any other affection fitted for public criticism. One of his volumes did not make its appearance till after his burial.

There can be no doubt that one of the safest tests of greatness proves Smellie—the verdict of posterity, the unanimous testimony of those best fitted to judge. He was not one of the mighty (or almighty) little contemporary Londoners: he had no fashionable practice, no physicianship to H.R.H., the goose that lays for the accoucheur the golden eggs. But he made a good income, was able to "lay by," and to retire on a competency. Above all, he did more for London and for the world than any other London accoucheur before or since. He is a good example to the clan obstetrical now and for evermore. He was a raw-boned Sawney withal, who had a smoking-cap, many "floots," and could amuse himself by painting in oil at his easel. His chief pleasures, however, were, practising midwifery, bad cases, assisting brother practitioners, pursuing the study of the science, and doing as the lamp above his door is said to have proclaimed,—"*Midwifery Taught here for Five Shillings.*" Honoured is the burial-place of William Smellie and of Euphemia Borland his spouse. It is in a quiet churchyard in his native county, and Dr M'Clintock has made pilgrimage to it and described it for our behoof. We heartily thank him.

Smellie's great fellow-countryman and contemporary and (we hope retrospectively and believe, his sincere) friend, W. Hunter, was, first, anatomist; second, obstetrical anatomist; third, obstetrician in grand fashionable practice, but certainly not spoiled by it. He was, we think, the greater man of the two, but Smellie was the greater obstetrician.

It requires little analysis of Smellie's works to find the sources of his greatness. Many have shown diligence and perseverance equal to his; very few have shown such appreciation of the true method, and still fewer have successfully winnowed out the fine wheat from all the inevitable chaff in their gathered sheaves. Hopeful young men should always take example from their best predecessors—diligence, perseverance, method, and that combination of knowledge and genius that picks the grains of wheat.

Smellie made observations. Are not his collections of cases there to testify to his great faculty of seeing what is to be seen—what is before the nose? Few can see what is "staring them in the face," because they look with prejudice, look through green spectacles. The greener kind is the most assured of being able to see and of actually seeing. Nearly blind they are, but they must take courage, and, following the example of Newton, look and look and look again, look with resolution to see it. "In a word," says Smellie, "I diligently attended to the course and operations of nature which occurred in my practice, regulating and improving myself by that infallible standard." That he did so, how powerfully do his works testify. And we may remark that there is as much to do now as in Smellie's time, and that our progress comes from doing as he did. How much mere "argle-bargle" has there been about the respective places of induction of premature labour, forceps, and podalic extraction? How distinctly have we to recognise the necessity for banishing all the argle-bargle and vain *ipse dixits*, and to note that our real progress has come solely from better acquaintance "with the infallible standard"? "Medicina tota," says Baglivi, "est in observationibus."

Smellie not only made observations, not only underwent the drudgery of harvesting cases; he knew also how to find the fruit. Let the reader carefully peruse Dr M'Clintock's annotations, and he will see how Smellie's editor recognises Smellie's keenness of eye in discerning how to make progress. In this point of view the editor's remarks are discriminating and just—laying stress as he does on Smellie's discovery of measurements, of relations, of changes of mutual relations of parts; in a word, of mechanism. The rules for the forceps are not only examples of discovery, as M'Clintock well points out, but illustrations also of how to discover. Smellie's words should be in every obstetrician's mind. After describing imperfect ways of using this noble instrument, he adds, "I began to consider the whole in a mechanical view, and reduce the extraction of the child to the rules of moving bodies in different directions. In consequence of this plan, I more accurately surveyed the dimensions and form of the pelvis, together with the figure of the child's head and the manner in which it passed along in natural labours; and from the knowledge of these things I not only delivered with greater ease and safety than before, but also had the satisfaction to find, in teaching, that I could convey a more distinct idea of the art in this mechanical light than in any other, and particularly give more sure and solid directions for applying the forceps, even to the conviction of many old practitioners when they reflected on the uncertainty attending the old method of application." "Daily reference," elsewhere says Smellie, "proves that we are still imperfect, and very far from the *ne plus ultra* of discovery in arts and sciences; though I hope every gentleman will despise and avoid the character of a selfish secret-monger."

We had marked several other matters for discussion in this notice, especially the secret use of the forceps which Smellie tries to justify on the ground of the novel character of the operation and the want of experience of the operator. We also wished to point out what Smellie evidently felt, the advantage that comes to the teacher himself from teaching others. Great accoucheurs are almost exclusively found among teachers! But we conclude abruptly by thanking the Irish editor for his appreciative edition of the work of a Scotchman, and for his excellent notices of his contemporaries, and especially of his Irish contemporaries, and of some Irish predecessors.

*The Collected Works of Dr P. M. Latham, with Memoir by Sir Thomas Watson, Bart.* Edited for the Society by ROBERT MARTIN, M.D., Cantab., Fellow of the Royal College of Physicians; late Physician to St Bartholomew's Hospital. In Two Volumes. Vol. I. The New Sydenham Society. London: 1876.

WE took occasion in our obituary of November 1875, when offering an imperfect sketch of Dr Latham, to commend very warmly his professional writings, then out of print, to those of our readers who had time and leisure to peruse them thoughtfully and carefully. It indicates discrimination and taste on the part of those who conduct the New Sydenham Society, that they have selected for publication a work so different in many respects from the ephemeral books which issue in such numbers from the teeming press of the present day.

The work is to consist of two volumes. The first, now published, contains thirty-eight lectures on the natural sounds of the heart, the various murmurs, the clinical history, and the treatment of the diseases. There is an excellent preface by the editor, Dr Martin, whose modesty and reverence for his old friend the author have prevented him from giving a few annotations which some may perhaps desiderate. But, on the whole, we are inclined to agree with him, that "it would be hardly possible to add much of absolutely certain knowledge to the grand broad outline which Dr Latham drew of heart disease—its general symptoms, and its physical signs." Let the reader study Latham's book, published thirty-two years ago, as it ought to be studied, and he will come to feel with Dr Martin, that any notes that could be added to the text must be mainly occupied with comparatively minute and unimportant details. Such things can hardly be taught; they are th acquisitions of individual experience and common sense. When written and printed, they are apt to bulk too largely in the reader's mind, and even to encumber him with imaginary knowledge—real to the writer, but imaginary to him. We have known, we think, such refinements lead to palpable and serious errors of diagnosis, and therefore of treatment.

The editor's suggestive preface is followed by an admirable *In Memoriam*, reprinted from St Bartholomew's Hospital Reports, by Sir Thomas Watson, than whom no other man is better able to form a just and thoroughly appreciative estimate of his friend and contemporary. His tribute to Dr Latham's memory, although short, contains everything essential, bringing the personal history, the professional attainments and progress, the moral character of the departed, in a very vivid and graceful manner before the mind's eye, and satisfying, we have no doubt, the warmest feelings of surviving friends and acquaintances. We must quote the concluding paragraphs of Sir Thomas Watson's estimate of his friend: "If I were called upon to sum up his character in the fewest words, I could use none more fitting than those with which he (anonymously) has commemorated his beloved brother John. He was 'an example of that peculiarly English character, the unobtrusive but accomplished and high-minded Christian gentleman.'

"He had outlived nearly all his contemporaries. Two yet remain, who, next to the persons of his immediate family, and beyond all others, were those between whom and himself the ties of ancient friendship and mutual affection were, I have reason to know, the strongest.

"Sir John Coleridge is one of these. Both of us, indeed, are now stranded—if of him I may presume to say so—upon the fast-narrowing sands of time.

"Ripe in years as he was, and ready in spirit for the solemn change, his death must long be the subject of tender and sacred regret among the nearest and dearest of his surviving family and friends; nor will his memory soon cease to be reverently cherished throughout a much wider circle.

'Multis ille bonis flebilis occidit,
Nulli flebilior quam mihi.'

The author's Preface is characteristic, and repays careful perusal. The following sentence shows that his own views were quite in accordance with those of Dr Martin on the matter of annotations: "My readers will be chiefly those who are seeking information at the bedside. To such there is no greater impediment of knowledge than over-teaching. The teaching which they most require is suggestive. They have the realities themselves to learn from, the original *book* to read, upon which all sound instruction is but a commentary. Therefore the commentator should only interpose when and where he is needed, and not after the manner of certain critics, who must *help* us with their annotations where the sense of the author is clear beyond dispute."

Our limits must prevent us from entering at any length upon a review of the book itself. It is one of the few which deserve to live, because it is full of real and conscientious work—of observations carefully, reverently, and modestly made during a long series of years—of thoughts pondered and repondered with candour and self-distrust and willingness to be taught, while the literary execution is unmistakably that of a man of education, culture, and taste. We might therefore content ourselves with recommending it strongly to all who can appreciate a really good book when it comes within their reach, and who rejoice in having important truths set before them in a manner fitted to strengthen, stimulate, and please their own mental faculties; but we shall direct attention to a few points which seem to be specially worthy of notice.

Pericarditis, we all know, was familiar to our predecessors even before the days of auscultation as a frequent concomitant of acute rheumatism; but endocarditis was a much later discovery, and Dr Latham had much to do with it. This is his own account of the discovery:—"In the year 1826 I was the first to teach the students of this hospital the fact, that whenever the heart was affected in acute rheumatism, a sound different from the sound of health always accompanied its contraction. This was then a new fact, and one of immense importance; and all succeeding observation has gone to confirm its truth. This sound, in the vast majority of instances, was the bellows' murmur. I must not call it endocardial, for it was not yet known to be so. But in some, instead of the bellows' murmur, it was a strange sound, difficult to describe; and in others this strange indescribable sound and the bellows' murmur seemed to occur together: there was a mixture of both.

"My notion *then* was, that all these sounds arose in some way or other out of inflammation of the pericardium; and taking them all, severally and in combination, as the signs of pericarditis, I was amazed to find how far the frequency of its occurrence in acute rheumatism exceeded the common calculation and belief.

"In process of time I found, upon a comparison of cases, that where in acute rheumatism the bellows' murmur occurred *alone*, the affection of the heart was upon the whole far less severe and far less perilous to life than where some unnatural sound occurred *alone* or in combination with it. I observed, too, that in some cases where the bellows' murmur was unequivocal, the patient betrayed no uneasiness, no palpitation—in short, no other symptom which could give the least suspicion of a diseased heart; yet that, in the great majority of instances where it once existed, it remained permanent as long as the patient continued under my care and observation.

"At length I began to doubt whether the bellows' murmur arising in the course of acute rheumatism was really derived from the pericardium, and to suspect that it proceeded from the internal lining. But for years the practice of this great hospital did not afford me a single opportunity of resolving my doubt or of confirming my conjecture. For of that disease of the heart, which, coming on during acute rheumatism, is characterized by the bellows' murmur, no patient of mine ever died, and I could learn nothing about it from dissection. But what my own experience would not furnish, M. Bouillaud has supplied. Many have died during the active progress of this disease under his care, and dissection has found it to be inflammation of the endocardium. Thus, we are indebted to M. Bouillaud for our first knowledge of this important fact."

Let us ask here how it came to pass that none of Dr Latham's patients with simple endocarditis died? In reply, we refer the reader to Chapters xiii., xiv., and xv., where incontrovertible evidence, in our opinion, is given to the efficacy of a judicious use of mercury in the treatment of the disease. It shows what those practitioners forego who doubt the remedial value of that remedy and others, because what is called advanced science has failed to discover their efficacy in dogs, in cats, and rabbits. There are men with scientific proclivities in medicine as well as theology who fall short of truth, because they lack reverence, modesty, and candour.

Dr Latham, after claiming his own share in the discovery of endocarditis, goes on to show that his distinguished contemporaries Watson and Stokes "farther illustrated this important subject by separating *those other* sounds of the heart which occur in acute rheumatism from the bellows' murmur, and analyzing them apart. In short, they showed these to be the proper signs of pericarditis, as the bellows' murmur is of endocarditis."

When this book was published in 1845, the treatment of acute rheumatism was different from what it is now, the idea of its being a blood-disease not being practically realized. How greatly the modern treatment, founded on that view of the pathology, has stript the disease of its formidable character will be admitted; and it is interesting to see how the large mind of Latham almost anticipated what has since taken place. P. 138—"Endocarditis and pericarditis are commonly spoken of as *incidental to* acute rheumatism. But beware of language, for it is often a great cheat. An incident is something that possibly may happen—a casualty. But whether endocarditis and pericarditis arise in two-thirds, or in one-half, or in not more than one-third, of all the cases of acute rheumatism which occur, still, to say of them that they are possible casualties, would lead people greatly to underrate their frequency, and greatly to underrate, too, the amount of peril which belongs to the disease, entirely from their being a part of it. Besides, to speak of them as being *incidental*, seems to settle at once that their relation to the rheumatism is of a certain kind. But who shall say that endocarditis and pericarditis are not equally *essential* to it with inflammation of the joints, and that both are not equally derived from the attendant fever? Or who shall say that the arthritic inflammation and the cardiac inflammation, and the fever itself, with its profuse and sour-smelling perspirations, and the urine loaded with lithates and red colouring matter, do not all spring from some noxious principle formed in, or finding its way into, the blood, this last containing, in truth, the *essence* of the disease? Organic chemistry alone can settle the point, and probably will settle it some time." We seem to read the words of a Vates Sacer.

We call attention to Lecture xvi. This is very suggestive, and must be read with much interest when we remember its date. It speaks of endocarditis independent of rheumatism, and specially points out a class of cases—which occur not unfrequently, we suspect—where the patient may labour under the affection for weeks and even months, and is often regarded as a hopeless subject of heart disease, when a prolonged and well-directed course of mercury might diminish if not remove the symptoms. One case is related in which endocarditis appeared to be the whole and sole disease, unaccompanied by rheumatism or any other known malady elsewhere in the body. A formidable group of symptoms, which had endured for two or three months, and were certainly on the increase, all referable to the heart, gradually disappeared, the murmur among the rest, under the use of remedies—leeches and mercury—addressed to the purpose of arresting inflammation.

We quote, in conclusion, the closing paragraph of Lecture xvii.

"The subjects of our profession require to be treated summarily or in detail, according to the degree of light that is brought to bear upon them from a general pathological principle. If you enter a spacious room with a small taper, you must carry it about, and pick your way with it into corners and recesses, and round pillars and projections, and after all you will hardly know where you are, and will be lucky if you escape accidents. But if you enter the same with a bright burning lamp, you have only to place it on a pedestal, and then stand in the midst and look around; and then you will find all things, great and small, near and remote, brought out equally to view, and will at once understand and admire the beauty and proportions of the whole apartment. So it is with our clinical inquiries. We must deal much in detail, we must note cases one by one, while we yet want a great pathological principle, which can show their natural relation and reconcile them together. But once establish such a principle, and it will compass and illustrate perhaps a hundred particulars at once, and render their minuter examination needless and superfluous.

*Traité de la Diphthérie.* Par. A. SANNÉ, Docteur en Médecine, Ancien Interne des Hôpitaux de Paris. Pp. 634. Paris: 1877.

DR SANNÉ thus introduces to the reader his Treatise on Diphtheria:—

"Since the date of the immortal work of Bretonneau, diphtheria has been the subject of numerous investigations and of publications of high interest. The most celebrated are the works of Trousseau, who added to the picture of the disease certain accessories which had escaped his predecessor. The really original part of the work of these two illustrious physicians—that relating to the specific nature of the disease—has maintained its position. Their theory has stood the test of time, has resisted powerful attacks, and has been re-accepted by a generation of physicians transiently shaken in its belief by speculations hastily reared on unreliable anatomical data. The malady has meanwhile kept ever on the wing, and has now reached nearly every part of the world. Its study has been perseveringly pursued; new views have been enunciated; and certain important phenomena have been at least carefully examined, though not yet perhaps completely elucidated. Under these circumstances, it has seemed to me a useful occupation to collect and classify scattered scientific researches, to make note of our acquisitions of knowledge, to explain the actual state of our information on disputed points, giving at the same time the results of my own researches, and so bringing together all that is known regarding diphtheria. This, the enterprise I have attempted, is one to which I have been irresistibly attracted, and towards which my studies have been for a long time directed. A pupil of Barthez and Trousseau, two masters who have largely contributed to the progress of this branch of pathology, I have also myself had opportunities of seeing diphtheria. In 1869 I made a scrutiny of an apparently very limited part of the subject—the results of tracheotomy—and pointed out the real place which that operation ought to occupy. The numerous works which I then only skimmed, so to speak, offered me in profusion desirable materials. To them have since been added an imposing number of cases taken by me at the Sainte-Eugénie Hospital, and in my private practice; and likewise notes taken by me of all the cases of diphtheria admitted to the wards of Dr Barthez from 1869 to 1874—notes which my illustrious master has allowed me to use, together with the notes of all his cases from 1854 to 1869. The following work is, therefore, the quintessence of nearly fifteen hundred cases. I have also derived numerous facts from a great many theses and memoirs upon diphtheria."

After the perusal of such an introduction, the intending reader of Dr Sanné's work will naturally be prepared for a compact and exhaustive treatise on diphtheria—a treatise combining a critical digest of the best writings on the subject, with the result or "quintessence" of the large clinical experience of an author thoroughly in love with his work, and trained for it by the teaching of Trousseau and Barthez, two of the most admirable masters who could be named. The expectations raised are high; but they are not likely to be disappointed. Dr Sanné has produced exactly the kind of work indicated in our extract from his introduction; and likewise the most complete book in any language on diphtheria. He has thereby not only made for himself a high reputation, but has likewise secured to the French school of medicine, for at least many years to come, a continuance in that foremost place of honour in the still advancing pathology and therapeutics of diphtheria which it has maintained since 1826, the date of Bretonneau's *Recherches sur l'Inflammation Spéciale du Tissu Muqueux, en particulier sur la Diphthérite.*

After defining the disease, and sketching its history in past times and in different countries, Dr Sanné devotes eighty-eight pages to a description of its pathological anatomy, discussing first the external character of the false-membrane, its structure, chemical characters, mode of evolution, and the condition of the mucous membrane or the skin on which it is deposited. He then describes the localisation of the false-membrane, and the various secondary organic lesions found after death. In this chapter he also treats of cardiac thrombosis. The next chapter is devoted to the symptoms of the disease—its course, duration, modes of determination, and relapses. Diphtheritic paralysis is fully considered. Then follow chapters respectively on the diagnosis, the etiology, the prognosis, and the treatment. Tracheotomy is considered in all its bearings, historical, statistical, and surgical, 172 pages being occupied with this subject. The remainder of the volume contains some remarks on tubage of the glottis; the treatment of certain symptoms, complications, and sequelæ; and a few suggestions on the subject of prophylaxis.

*The diphtheritic membrane*—the false-membrane characteristic of the disease—in the author's opinion, has for its basis a fibrinous exudation. He shows that it is not the product of a degeneration of the epithelium; and that the theory of its parasitic origin, at present in favour with some German and Italian physicians, though specious, is entirely untenable. The formative process is always the same; but the character of the false-membrane is modified by the intensity of the exudation (which he speaks of as "inflammatory"), and the structure of the particular part of the mucous surface upon which it is deposited. He says:—

"The inflamed mucous membrane presents, among other changes, these well-known alterations of vascular surfaces, which, according to the generally received opinions of Cohnheim, allow emigration of leucocytes and a fibrinous exudation to take place. This point being established, it becomes easy to see how the product may vary with the varying intensity of its cause. When the inflammation is slight, an exudation takes place on the surface of the mucous membrane, which itself becomes infiltrated with young cellules. The lesion, being superficial and of little intensity, undergoes separation without leaving a cicatrix. When there is a somewhat greater intensity of the cause, the exudation is deeper, and the vitality of the mucous membrane suffers: elimination is followed by a slight loss of substance. Finally, in cases in which there is that high degree of intensity, which the Germans designate 'the diphtheritic form,' the inflammation is energetic: the exudation of fibrin and young cells is so deep and dense as to strangle the circulation in the invaded parts. The result is their mortification, their assumption of an ashy grey or a brown colour, and their subsequently comporting themselves as sloughs.

" *The structure of the mucous membrane* equally imparts a special character to the product of its inflammation. On the pharynx and sub-glottic portion of the larynx, the epithelium is thick, and composed of pavement-cells: it closely adheres to the mucous chorion. This anatomical disposition favours a deep-seated infiltration of the tissues: it explains why the pharyngeal false-membrane is superficial and slightly adherent in those cases in which the exudative process is moderate, and thick and tenacious under the opposite conditions; and how, without seeming to be gangrenous, it may, on falling off, cause loss of the substance of the mucous membrane. In the portion of the larynx below the glottis, on the contrary, as well as on the whole of the arborification of the air-tubes, the epithelium is composed of cylindrical cells which are much thinner; but that which chiefly changes the anatomical conditions is the existence of the basement-membrane of Bowman, an amorphous stratum which, by separating the epithelium from the chorion, forms a barrier difficult to pass. The study of this anatomical arrangement explains the generally superficial character and feeble adherence of the false-membrane which is produced on the parts in question."—Pp. 46, 47.

*The chemical elements of the diphtheritic product* are stated to be four in number, viz., fibrin, an amorphous material, fatty matters (in great abundance), and mucus. The author adds, that, according to Robin, false-membrane is formed by an exudation of plasma, which separates into two parts—a liquid which flows away, and another part which coagulates in the form of fibrin, and so produces membrane. The effect of numerous reagents is described. The information given is not only in itself interesting; but it will be useful in pointing out to practitioners the substances, and combinations of substances, which may be advantageously or safely employed to dissolve or separate the false-membrane during life. The action of *water* is feeble. Diphtheritic pellicle, after being immersed in water for three or four days, is disintegrated, but in no degree dissolved. *Alcohol* dissolves the fatty matter, leaving the membrane hard and shrivelled. *Glycerine* swells false-membrane, and renders it transparent. *Iodine*, in the form of tincture, imparts to it a yellow colour and hardness. An aqueous solution of *bromine* renders false-membranes hard, friable, and disintegrated. They are acted on energetically by nearly all *the acids*. The action of *lactic acid* is thus described :—" It has been studied by Adrian and Bricheteau, who state that its action is more radical than that of the other acids. Two drops dissolved in five grammes of water, in a few seconds reduces the false-membrane to a transparent web; and at the end of ten minutes nothing visible remains in the liquid except some hardly perceptible fragments of a frothy gelatinous substance. Upon the addition of some drops of the acid, all trace of solid substance will disappear, but there will always remain a slight cloudiness. I repeated the experiments with the solution recommended by the two authors—five grammes of lactic acid to one hundred grammes of water. The false-membrane very rapidly became thin, but the stringy web [*trame fibrillaire*] remained, although I gradually increased the quantity of lactic acid to the proportion of fifteen grammes to the hundred grammes of water. The thickness of the false-membrane on which I experimented was one centimeter and a half. The difference in my result is a matter of secondary importance: the principal fact is corroborated, viz., that lactic acid in a few minutes reduces false-membrane to so thin a web as to be quite insignificant as a local lesion." Regarding *lime water*, the best and most rapid solvent of false-membrane, Dr Sanné says :—" Its solvent power was pointed out by Küchenmeister. I have often verified this power with test-tube in hand. I have plunged into a graduated tube, containing six cube centimeters of lime water, a piece of false-membrane of from one to two centimeters in thickness: the water immediately became turbid, and the membrane became rapidly thinner. In ten minutes there remains only a transparent web, which after about half an hour—sometimes a little more and sometimes a little less—has disappeared. The liquid becomes rather turbid, but not to such a degree as to prevent the remainder of the false-membrane from being easily distinguished. Next day it is clear, and there is a white deposit at the bottom of the tube."

The solvent action of a reagent is not always equally rapid. This diversity depends upon the structure of the membrane. The false-membrane of a gangrenous sore-throat, which contains, together with the fibrin, fragments of mortified tissue, will to a great extent remain undissolved, or will at least be little influenced by solvents. There is also in purely fibrinous membranes different degrees of solubility.

Nitrate of silver does not dissolve false-membrane, but causes it to contract and shrink. Perchloride of iron produces no direct effect: when it comes into contact with organic matter, it undergoes decomposition into hydrochloric acid and oxide of iron, the acid being set free, and the oxide deposited in a state which admits of being removed by scraping.

*The evolution of false-membrane* is ably discussed as a question of pathological physiology. He says:—"The mucous membrane, when in travail with diphtheritic membrane, becomes the seat of inflammatory action, which, though special in its origin and in its course, presents nevertheless all the known characters of phlegmasia of mucous surfaces. The prominent feature of this elaboration is a transudation from the vessels of a fibrinoid substance with which numerous emigrant leucocytes become united. On its first appearance, the false-membrane is thin, soft, and semi-transparent. The German school has maintained, and still holds, that this kind of false-membrane is peculiar to the air-passages, and is never, or almost never, seen in the throat. . . . . In a very brief space of time, generally within some hours, the exudation going on, the false-membrane increases in thickness, becomes harder, and attains its complete state. At the same time, its surface extends. It seldom takes at a bound its definitive dimensions: it commences with a rounded patch, often very narrow, or even punctiform, which assumes an eccentric development. This mode of extension is sometimes very remarkable. At first there are only one or several little points, exceedingly similar in appearance to the vesicles of herpes, or to concretions on the tonsils. They enlarge, coalesce, and ultimately form one or two very characteristic diphtheritic membranes, such as are met with in bad cases of pharyngeal diphtheria [*angines graves*] and laryngeal diphtheria [*croup*]. I have seen a great many cases of infectious sore-throat [*angine infectieuse*] commence in this way—both those which have and those which have not been followed by laryngeal diphtheria [*croup*]. The pseudo-membranous inflammation, which begins on one point or on several at the same time, extends by contiguity, and very often shows a remarkable tendency to propagate itself from above downwards. Bretonneau, who was struck with this peculiarity, explained it by saying that the primarily affected parts yielded an acrid, virulent, epispastic fluid, which, by running down, irritated the parts below, and communicated to them the pseudo-membranous inflammation. This hypothesis becomes untenable when facts are deliberately examined. The existence of the acrid fluid is more than doubtful. Moreover, the advance of the diphtheritic exudation is much less generally descending than Bretonneau supposed: the cases of ascending laryngeal diphtheria [*croup*], coryza, and otitis, consecutive to sore-throats [*angines*], are common. One cannot see what action the acrid fluid—supposing it to be a reality—can have in the circumstances: without taking into account the influence of the movements of deglutition carrying it to the œsophagus, the mucous membrane of that canal ought to be one of the worst-treated parts; but the very opposite of this is the fact, the œsophagus being so seldom attacked with diphtheria that the presence of deposits of false-membrane on its surface has been explicitly denied. Moreover, false-membrane does not always originate in one spot: it is not unusual to see the exudation commence at the same time in several places, separated by healthy spaces. The theory now referred to gave rise to a special therapeutic system, which consisted in cauterizing freely—pitilessly—with savage energy [*avec une sauvage energie*]—the localities affected by diphtheritic exudation, with the object of concentrating and destroying the morbid poison in its place of origin."

The extract now made is important in a practical point of view, showing as it does the baseless nature of the theory on which Trousseau rested his system of cauterization, and advocated the primary importance "*de détruire sur place le principe morbide.*" We call particular attention to Dr Sanné's strong reprobation of the practice based upon that doctrine, for we believe it is still in vogue, in a more or less modified degree, with the British admirers of Trousseau; though it has of late years been becoming more and more discredited in France, and is now nearly an obsolete tradition in the hospitals of Paris.

*The lesions of the mucous membrane* in relation to the diphtheritic exudation is the next subject discussed by the author. After briefly referring to the views of French physicians, and the attempt made in Germany to restore the pre-Bretonneau hypotheses, Dr Sanné says:

"In my opinion, the mucous membrane behaves differently in simple cases, and in the cases in which the infectious element preponderates. French authors have written that the state of the mucous surface and the appearance of the false-membrane vary according to the diphtheria being primary or secondary: they have consequently adopted this division as the basis of their description. I do not admit this view to be correct, for the differences in question belong essentially, not to the primary or secondary nature of the disease, but to its degree of intensity, and to its nature being simple or infectious. If certain formidable local characters often occur in aggravated form in secondary diphtheria, they are not peculiar to it, being only too frequently seen in the primary form of the disease. It is rational, therefore, to examine the modifications which the mucous and sub-mucous tissues undergo in simple cases, as well as in those which are intensely severe and infectious. At first, the mucous membrane is red and hyperæmic: in some cases, according to Daviot, it has a transparent, œdematous appearance. The capillaries are dilated and arborized, or they form small point-like spots. Their walls become altered, so as to allow transudation of leucocytes and fibrin. The leucocytes impregnate the mucous tissue to a considerable depth, and they extend over a larger surface than that occupied by the false-membrane. The distribution of the fibrin differs according to the cases, and particularly in respect to the intensity of the *processus.* In mild forms of the disease, it is deposited upon the surface of the epithelium as a white pellicle which acquires thickness and hardness. In severe forms, and in particular regions, the fibrin infiltrates superficially or deeply, the chorion arrests the nutrition of the parts, and so leads to their gangrene, sloughs being formed, which are eliminated in the usual way. When the formation of the false-membrane has taken place, the mucous membrane continues to be congested for some time, but it generally remains smooth. The mucous membrane and its morbid product are so strongly adherent to one another that they cannot be separated easily or without slight bleeding. Little by little the congestion diminishes, the mucous membrane returns to its normal state, and the false-membrane separates, taking with it much of the epithelium, which is replaced in a short time. In cases of greater intensity, the congestion is accompanied by swelling and infiltration of the mucous membrane, and of the connective tissue in a certain zone surrounding the false-membrane, which has the appearance of being situated at the bottom of a hollow. The mucous surface is wrinkled and rough; on it ecchymoses form, which likewise imbibe false-membrane and stain it brown."

"Ulcerations of various forms and dimensions are met with on the surface of the mucous membrane. They are sometimes round, and sometimes sinuous, resembling, according to the comparison of Barthez and Rilliet, the trail-mark left on a sheet by worms; their edges are as if hewn out, and are not deglutinated. The bottom of the ulcer is formed by muscular fibre, which is often altered, as recent researches have shown, in opposition to the opinion formerly entertained. In very severe and infectious forms there is a great amount of swelling and œdema; the ecchymotic coloration is deep, and there is a more extensive transudation of fibrin and leucocytes into the chorion of the mucous membrane. The surface of the mucous membrane becomes uneven and fretted: it softens, mortifies, and becomes covered with large ulcerations, which are gray or brown-coloured, and have deglutinated edges smeared with a greyish-green detritus. The ulcerations exhale the fœtid odour characteristic of gangrene. The tonsils, uvula, veil and pillars of the palate, are the parts which most frequently mortify. The mucous membrane of the larynx is much less frequently affected in that way."

"The necrosic process varies in intensity. In most cases it only involves a portion of the thickness of the mucous membrane, but in others it invades the subjacent connective tissue, and even the neighbouring muscles. In diphtheritic gangrenous sore-throat, I have seen the separation of the sloughs show numerous perforations of the veil of the palate. As the illness had neither been preceded nor accompanied by measles, or other morbid condition tending to gangrene, the diphtheria alone was responsible for the unusual intensity of the gangrene. Cases have been observed in which the mortification has advanced, step by step, to the vicinity of the large vessels. In the less severe cases, the ulcerations heal, but leave in their place a cicatricial tissue, the contraction of which causes deformities and strictures, which may seriously compromise the functions of the pharynx, larynx, and trachea."

We have quoted nearly *in extenso* the author's admirable account of the formation of the specific pellicle, and of the lesions of the mucous membrane therewith associated, because we have not elsewhere seen the subject so lucidly explained; and also, because it is only by possessing a correct and clear knowledge of these subjects that the physician can treat cases of diphtheria in a scientific manner, and rationally prognosticate their issue.

The specific pellicle appears on the skin most commonly in situations where it has become denuded of its superficial epidermis, or where there are ulcers, excoriations, and eruptions. The alterations which take place in the skin are similar to those already described as affecting the mucous membrane. The mode of formation and the structure of the false-membrane are the same on the cutaneous as on the mucous surfaces.

In discussing the *lesions of organs*, the author enters a dissent from Bouchut and Labadie-Lagrave's statement that endocarditis is a frequent accompaniment of diphtheria; and in respect to this question he adheres to Parrot's refutation of the views set forth by these two physicians. According to Bouchut and Labadie-Lagrave, endocarditis is a common sequel of diphtheria. In a second memoir published on this subject, Labadie-Lagrave concludes from twenty autopsies that diphtheria, irrespective of its form and the locality of its manifestation, nearly always brings in its train acute endocarditis, particularly inflammation of the serous covering of the auriculo-ventricular valves. Dr Sanné argues that this is a mistake originating in a misconception of the two anatomical characters adduced in proof of the frequency of the lesion. He agrees with Parrot that the pretended recent inflammatory lesions are due to fibrinous transformation of small hæmatomes, or warty stalactites, developed upon the valves during very early life, perhaps even during intra-uterine existence.

*Cardiac clots* and *cardiac thrombosis* do not, in Dr Sanné's opinion, play as important a part in the mechanism of death as has been assigned to them by Richardson, Labadie-Lagrave, and others.

*The fever* in diphtheria is described at considerable length. The great facts are, that the poison of diphtheria is only moderately pyrogenic, and that the degree of fever met with is exceedingly variable. In acute cases, when the malady is in full career, the pulse ranges as a rule from 120 to 140, and even rises higher. Death in malignant cases is often preceded by a great and sudden fall in the pulse. As Wunderlich has shown, there is no disease in which temperature has so little significance. The correct remark of H. Roger is quoted by Dr Sanné to the effect that the animal heat is increased in laryngeal diphtheria, but to a much less extent than might be anticipated from the rapidity of the pulse and the greatly accelerated respiration.

*Diphtheritic paralysis*, in the opinion of the author, depends upon peripheric alterations of the nerves, and not on central lesions of the nervous system. He does not admit that the doctrine of asthenia satisfactorily explains the supervention of paralytic attacks during or after apparent convalescence. He says: "The sore-throat which has preceded the paralytic affection has been mild in character; there has been little fever; the rise of temperature has been insignificant; the patient has been alimented and subjected to a tonic regimen; nothing has occurred to indicate asthenia—that is to say, general exhaustion from waste of tissues. The same remark is made by Hermann-Weber. He shows that English physicians employ largely, in the treatment of diphtheria, wine, tonics, and alimentation, a method which does not prevent the occurrence of a large number of secondary paralytic affections, even in slight cases of the disease. I do not argue that asthenia has never any effect. Its action is too evident in numerous cases of diphtheria to allow of its being denied. It ought, however, to be regarded only as an assisting cause."

Probably many of our readers will agree with us in thinking that Dr Sanné has, in the preceding extract, considerably underestimated the influence of asthenia in causing the paralytic affections of diphtheria. Fully admitting the operation of other causes, we agree with those who assign to asthenia a principal part in the production and maintenance of paralytic states in diphtheritic patients. Hermann-Weber's remark upon the non-preventive power of the wine, tonics, and food so freely administered by English physicians, has really very little bearing on the question at issue, which is the *assimilation* and not the mere administration of food. Since that mischievous empiricism called "Todd's system" came so much into vogue in England, asthenic patients—and many other patients—have been too often over-fed and over-stimulated; their defective powers of assimilation not being sufficiently considered in the treatment. Administration of food is not necessarily alimentation.

*Albuminuria* is stated to be a very common epiphenomenon in diphtheria, "one which, in the great majority of cases, does not influence the course of the disease." The author has never known a case in which it became chronic. The non-occurrence of dropsy in the albuminuria of diphtheria he explains by the fact that generally one only of the kidneys is affected—not both, as in scarlet fever. There is, no doubt, congestion of the kidneys in common with other visceral congestions in the acute stage of diphtheria, but the occasional very early appearance of albuminuria—its intermittances, its varying intensity at different periods of the twenty-four hours, and its never becoming chronic—lead us to think that it has far more relation to waste of tissues and non-assimilation of food than to lesion of one of the kidneys. That, however, is not the opinion of the author.

Passing over many interesting and admirable pages, we come to a discussion on the question now before the profession in England, and in particular before a Committee of the Royal Medical and Chirurgical Society of London, viz.—*Is there a non-diphtheritic membranous croup?* Dr Sanné unhesitatingly answers this question in the negative, as it has already been answered by most French authors, by an increasing number of British authors, and Sir John Rose Cormack and Dr Turner recently in our own pages. Notwithstanding the interest of the subject, we must touch it lightly, a considerable amount of space having been so recently devoted to it.

Dr Sanné, like most French physicians who read about "croup" in the English acceptation of the term, confesses to bewilderment. After quoting Dr Archambault's translation of Dr Charles West's description of croup, Dr Sanné says—"It is difficult to gather from this passage the exact belief of the English physician. Does he mean to speak of two different kinds of pseudo-membranous laryngitis? In the second of these two diseases he evidently sees infectious croup. To what pathological type can the first be adapted? It is applicable to at least two morbid states which the author seems to confound—stridulous laryngitis and membranous croup appearing at once in the larynx or trachea without previous manifestation elsewhere, and without dominance of the poison-element [*croup d'emblée sans intoxication dominante*]. There might be apportioned to stridulous laryngitis most of the characters with which he endows the first disease. As is well known, stridulous laryngitis develops itself as a catarrhal affection—only attacks children, their liability to it depending on their narrowness of larynx—is not epidemic—is not contagious—and is prevalent almost exclusively in winter."—P. 297.

Some of the confusion on the subject of croup which pervades British medical literature is probably due to the slovenly manner in which the pamphlets of Home and Cheyne were examined and used by the earlier teachers and compilers of systematic treatises. Subsequent authors have repeated the representations of their predecessors, sometimes altering and sometimes amplifying them; but few of our later writers appear to have examined for themselves the tracts on which they base much of their discussions. If the short papers of Starr, Home, Cheyne, and a few others, were reprinted, without note or comment, a cheap and useful contribution to the literary history of diphtheria would be furnished; and all inquirers would be able to judge for themselves of the exact value and import of writings which are constantly being inaccurately quoted, and which can at present be consulted only in a few great medical libraries.

The volume of the Philosophical Transactions for 1750 contains Starr's description of an epidemic of laryngeal and tracheal diphtheria which prevailed in Cornwall in 1748. F. Ruette, who published at Paris in 1809 a French translation of this essay, states in a preface that Home must have been acquainted with Starr's description, and must have derived from it all that there is original in his pamphlet, except the name which he gives to the disease—"croup."

In 1765, Dr Francis Home published at Edinburgh his much-quoted pamphlet, entitled, "An Inquiry into the Nature, Cause, and Cure of the Croup." It consists of sixty sparse octavo pages, and is chiefly made up of twelve illustrative cases, four of which were communicated by friends. Of Home's own eight cases, four are cases of tracheal diphtheria; three are cases of infantile stridulous laryngitis; and the description of one is not sufficient to fix its nature. The four communicated cases are cases of tracheal diphtheria. Home, therefore, like many of his successors, confounded together two entirely different diseases, viz., tracheal diphtheria and infantile stridulous laryngitis.

In 1801, Dr John Cheyne published at Edinburgh his "Essay on Cynanche Trachealis or Croup." It consists of eighty sparse octavo pages, illustrated by five excellent coloured plates, the work of an admirable draughtsman, our immortal physiologist Charles Bell. Cheyne distinguishes between the disease now known as laryngismus stridulus and other affections accompanied by stridulous breathing; but the cases which he describes under the designation of *croup* are cases of two distinct diseases, viz., tracheal diphtheria and infantile stridulous laryngitis. He, therefore, repeats Home's mistake of confounding the two diseases into one disease. There are ten cases described in Cheyne's Essay, three of which were communicated to him by friends. Five of the ten cases proved fatal, and the false-membrane which was formed in the air-passages of four of these five fatal cases is well described, and is also well represented in the plates. These four cases are cases of tracheal diphtheria. Five of the ten cases terminated in recovery, in none of which false-membrane is mentioned as having been formed. They are cases of infantile stridulous laryngitis.

(*To be continued.*)

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## *Diseases of the Nervous System.* By WM. A. HAMMOND, M.D. Sixth Edition. New York: 1876.

WHEN a book reaches the sixth edition, and that within five years, it is a sufficient test of its usefulness, and little comment will be required from us. The short introductory chapter on the instruments and apparatus, including the ophthalmoscope, æsthesiometer, and electrical apparatus employed in the diagnosis and treatment of nervous diseases, will be found useful. The treatment indicated for many disorders is based on sound physiological and therapeutical data; and here we would mention the use of the constant current in cases of cerebral congestion, which the author finds to be of the greatest service in many cases, "a few applications being often sufficient to abolish the vertigo and unpleasant feelings in the head, and to restore mental and physical activity;" and here also ergot, from its well-known effect on the vessels, has proved of service. A new remedy advocated—viz., paullinia—we do not remember to have heard of before.

Regarding the effects of overwork, or work of any kind during any cerebral affection, the following remarks are very *apropos:* "Men who readily see the impropriety of walking three or four miles while suffering with an inflamed knee-joint, do not hesitate to exert a disordered brain to the extreme limit of its power."

Another drug, whose use in practical medicine has obtained a wider field of usefulness since its action was investigated in the lower animals, is the nitrite of amyl, which is recommended very strongly in cases of cerebral anæmia, no deleterious effects resulting even when its use was continued for a year. This is a most convincing proof of the direct value to practical medicine of physiological research; for the action of this drug has been clearly proved to be on the muscular fibres of small arteries.

The important variations in temperature that occur during cerebral hæmorrhage are, we think, often not sufficiently attended to by the practitioner, and for him, at p. 71, some useful data are indicated. The apoplectic constitution, to which many of the older physicians attached so much importance, is not recognised by the author. Nor is the author inclined to ascribe so much importance to miliary aneurisms in the cerebral vessels as is ascribed to them by their discoverer Charcot. We cannot agree with the author, however, when he asserts that the function of the cerebellum is not materially different from that of the cerebrum. In cerebral congestion, very active effects are claimed for the bromides, especially those of lithium. The advice at p. 107 regarding the use of the induced current in hemiplegia is valuable.

The chapter on Aphasia is very good, the historical side of the subject has been well worked up, and the recorded cases are not the least valuable part of the chapter.

A valuable section is that one on toxic diseases of the nervous system, including plumbism, alcoholism, bromism, etc.

A large number of excellent woodcuts illustrate the work. The coloured plate, showing a transverse section of the cord, shows one how deficient is our knowledge of the functions of great tracts of the cord; and we are convinced that but little progress will be made in this direction until systematic sections by carefully graduated instruments are made, such as the recent excellent researches of Woroschiloff, made in Ludwig's laboratory, giving precision to the experiment and definiteness in the results. The practitioner will find this work a most useful repertorium of information and advice. There is, however, a tendency to speak very dogmatically on many even difficult subjects; and when one remembers how many things are so uncertain in medicine, it may occur to some that there is more than the usual effects claimed for some remedies, which certainly in the hands of some practitioners have not proved so serviceable as they seem to have done in the cases of Dr Hammond. In this book there is so much information drawn directly from the author's own experience that it cannot fail to be found of the greatest value to the practitioner, to whom we cordially recommend it.

*A Course of Elementary Practical Physiology.* By M. FOSTER, M.D., F.R.S.; assisted by J. N. LANGLEY, B.A. Macmillan & Co.: 1876.

THE authors believe that the study of structure and function ought to go hand in hand, and indeed much is to be said for this view; but the student of medicine with but scanty time at his disposal has to economize that time to the greatest extent, and assuredly makes the most progress in a department of physiology when his mind is kept constantly directed in one train of ideas. In this book the structure of the various organs is first indicated, then their functions and chemical composition are discussed. The first lesson is devoted to the dissection of the rabbit and dog, in which attention is paid only to details of physiological importance. The next lesson is devoted to the structure of the blood, and some experiments on coagulation are described. The various tissues and organs, their structure and functions, are then treated of *seriatim;* and a very valuable chapter on the dissection of the brain of the dog or sheep forms the last or twenty-seventh lesson.

There is an appendix containing directions for the preparation of various staining and chemical fluids, imbedding mixtures, etc. We fail to find any directions for the student wishing to make sections with a microtome. The book is clearly printed, and the large type indicating the subject is very useful.

The lesson on vision will be found very valuable, containing, as it does, many simple and easily performed experiments on the eye. It is important to note that everything described can be done *without* a certificate, such as is required by the recent Vivisection Act.

*A Course of Practical Histology.* By ED. ALBERT SCHÄFER, University College, London. Smith, Elder & Co.: 1877.

THIS is in many respects a really valuable book, and is devoted not to descriptions of tissues, but to those methods which the student has to apply in the laboratory in order to acquire a practical knowledge of histology, for the day has long gone by when it was fondly, though delusively, imagined that a knowledge of structure was to be obtained by merely looking at a demonstration of prepared microscopic sections. The order followed is that of "Quain's Anatomy," eighth edition, so that any one possessed of this book can easily work his way through the histology of "Quain."

A short chapter is devoted to the Microscope, avoiding details. The blood, epithelial, connective and other tissues, and then the various organs are considered, and the best methods of preparing them for permanent preservation are pointed out. The descriptions are all very clearly put, so that no student can fail to apprehend what is meant. Especially good are the woodcuts, done by Collins.

There is a short appendix, containing instructions for determining the magnifying power of a microscope, counting the blood-corpuscles, drawing with a camera, etc. Any one who follows the instructions given may very soon acquire a very perfect knowledge of the most modern and most useful methods employed in histological investigations. The attention to minutiæ, upon which so much of the success in histology depends, clearly shows that all the methods have been tested by the author. We can confidently recommend this work to the students as a good, reliable, and excellent guide to histological work in the laboratory or in private.

*Compendium of Histology.* Twenty-four Lectures. By Professor H. FREY. Translated from the German by GEO. R. CUTLER, M.D. Smith, Elder & Co.: 1876.

THE works of Professor Frey are very much better known in Germany than in this country; and in England, indeed, we are indebted for the translation of Professor Frey's works to Americans. This book professes to give a short compend of the most essential facts desirable for students and practising physicians. It is short, pithily and concisely written, and offers as far as it goes a good *resumé* of the leading facts in histology.

On the very first page we are struck with the figure of Bathybius, a form which owes its very existence to the imagination of Professor Huxley. The recent Challenger expedition has shown most conclusively that this much-lauded lowly form must be relegated to the domain of mythology. The general history of cell-life occupies the first lecture. The author might very conveniently, and with advantage too, have used the words epi, meso, and hypoblast instead of the clumsy translation of the corresponding German names—horn layer, middle germinal layer, and intestinal-gland layer.

The book is extremely well illustrated, and the figures will be found of great advantage to the student, but fig. 37, of "pricklecells," is entirely misleading. The prickles on these cells are represented as dovetailing into each other. Now it has been conclusively shown, that these prickles do *not* dovetail, but their edges are apposed, so as to leave channels between the cells. Further, in fig. 33, viz. ciliated epithelial cells, the clear band on which the cilia rest is not sufficiently clearly indicated.

The chapter on Cartilage is very clearly put, for very often the student has a most imperfect notion regarding the formation and destiny of the so-called "cartilage capsules."

The author does not believe in the distinction drawn by Kölliker between osteoblasts and osteoclasts—he recognises transition forms between the two; nor does he believe in the bone-destroying functions ascribed to the latter by Kölliker.

We are glad to see the name of Waller associated with Cohnheim on the subject of diapedesis. A very useful part of the book is the short history given of the development of each tissue. A peculiarity which, however, occurs in other American works, such as those of Dr Draper, is the dropping of the *u* in such words as to harbor, to favor, colored, etc. Some very excellent figures of the lymphatic structures are given, a subject to which Professor Frey has devoted special attention. The chapters on the spinal cord, cerebrum, and cerebellum are not so good as many of the others; indeed, no figure is given of the structure of the cerebrum, and the one of spinal cord is misleading. The cochlea is disposed of summarily, but the chapters on the Eye are good.

Here and there, there are instances of a too literal translation, but such defects can easily be remedied in a second edition.

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## *Virchow Archiv, Ausgegeben am 26th October* 1876. *Disease of the Marrow of the Bones in Pernicious Anæmia.* By J. COHNHEIM of Breslau.

THIS is a short communication, in the form of a letter to the Editor, in which Professor Cohnheim describes certain marked changes which he found in the marrow of a strongly-built man who had died of the disease now known as "progressive pernicious anæmia." The disease had developed itself gradually and without any known cause, and the patient had presented during life all the characteristic symptoms. After death there were found, along with general anæmia of the organs, punctiform extravasations of blood in many parts, such as the brain, the retina, peri- and endo-cardium, bladder, etc., marked fatty degeneration with some hypertrophy of the heart, œdema of the lungs, induration and hypertrophy of the spleen, and some enlargement of the liver, which was slightly jaundiced and very fatty. Equally striking were the changes in the osseous system. The marrow of all the bones, not only that of the skull, the sternum, and the ribs, but also that of the long bones, in their diaphyses as well as their epiphyses, was of an intensely red colour, not due to hæmorrhage. When examined with the microscope, this marrow showed a complete absence of the usually abundant fat cells. Instead of these, there were, *first,* numerous colourless corpuscles—the so-called marrow cells, some like lymph corpuscles, but many of large size, with one or two bladder-like nuclei, and along with these there were a few multinuclear giant-cells; *second*, there were, in at least equal quantity, coloured elements. Among these latter there was only a small number of biconcave blood-corpuscles; there were in greater proportion spherical red corpuscles, without nuclei, and varying in size from the diameter of a red to twice that of a colourless corpuscle. The coloured cells which were most abundant were red nucleated cells. These also were of different sizes, but, on the average, did not exceed that of the smaller epithelioid marrow cells. Their nuclei, which were of the same yellow tint as the protoplasm, were of different sizes, and were generally single, but in some instances double. Their bodies were possessed of great elasticity, for they could be drawn out to a fine point, and, if flattened beneath the covering glass, they at once regained their spherical shape when the pressure was withdrawn. On the whole, therefore, the marrow was thus reduced to a condition similar to that which it normally presents in the bones of the fœtus.

In the spleen, Professor Cohnheim found some red nucleated corpuscles similar to the above, but not more of them, he says, than that organ should naturally contain. He found a few also in the clots taken from the vessels after death, but he remarks, that although the blood had been repeatedly examined during the life of the patient, none had been then found in it. The chief abnormality observed during life was the presence between the normal blood-corpuscles of the small corpuscles described by Quincke and Eichhorst. There was no disturbance of the proportion between the red and white corpuscles.

The author's reflections upon this case are interesting. He believes it now to be established that besides the ordinary forms of anæmia, due to such causes as repeated loss of blood, or the draining away of nutritive material by excessive discharges, or its absorption by morbid growth, there is another form, exemplified by the pernicious anæmia, in which the root of the mischief is disease of the blood-forming cells. Among these cells recent investigation would seem to indicate that we must now class those of the marrow, and it would be a striking confirmation of this opinion if in all cases of pernicious anæmia changes were found such as those above described. The author, however, wisely refuses, as yet, to commit himself to any general conclusion. As circumstances render it improbable that other cases of the disease will come under his observation, he merely puts this case upon record with a view of directing the attention of other pathologists to the subject. He believes that further observation will soon render it apparent whether or not there is a constant association of such changes in the marrow with the "progressive pernicious anæmia."

*Medicinal Plants; being Descriptions, with Original Figures, of the Principal Plants Employed in Medicine, and an Account of their Properties and Uses.* By ROBERT BENTLEY, F.L.S., Professor of Botany, King's College, London; and HENRY TRIMEN, M.B., F.L.S. Parts 8–16. London: J. & A. Churchill: 1876.

WE have on two previous occasions directed the attention of our readers to this excellent work on medicinal plants. The numbers before us show that the work is still being executed with that care which characterized the earlier parts. When completed, it will be a most valuable work on vegetable materia medica. We wish it a wide circulation.

*The Druggists' Advertiser: Devoted to the Interests of the Retail Druggists, United States.* Published monthly by the Druggists' Advertiser Publishing Company. New York: 1876.

THIS monthly periodical has an extensive circulation amongst druggists in America.

It is well conducted; the articles contained in it being generally succinct, interesting, and calculated to be of much use to the profession.

The subjects are on the whole well selected, and this Journal is deserving of a wide circulation.

# Part Third.

## MEETINGS OF SOCIETIES.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION LVI.—MEETING IV.

*Wednesday*, 17th *January* 1877.—Dr GILLESPIE, *President*, and afterwards Dr KEILLER, *Vice-President, in the Chair.*

### EXHIBITION OF PATIENTS.

I. *Mr Annandale* showed—(1.) AN ENTIRE TONGUE removed for epithelioma a week ago. The patient from whom it had been taken was also shown, as he had been going about the ward for two or three days. In the removal, he had used the new thermal cautery of Paquelin, by which the patient suffered no pain after the operation. The good result he believed to be due to the thermal cautery used, the confinement of the disease to the tongue, and the fair health of the patient.

(2.) A PATIENT in regard to whose case he would be glad to have the opinion of any of the members. The man had been admitted to his wards suffering from complete motor paralysis of the entire arm due to the manipulations of a bone-setter who had tried to reduce a dislocation of the shoulder by making the elbow of the affected arm touch the opposite shoulder. By this manipulation he dislocated the elbow, and caused paralysis of the whole arm, rendering it perfectly useless, the fingers being slightly flexed, and the skin painful and glistening. Galvanism was tried for a month without any effect, so that the patient wished the arm removed. The question to be settled was, where should this be done? He himself at first thought the operation should be performed at the shoulder, but he ultimately amputated just above the elbow, as he wanted to leave a stump. Since the operation the man had been able to move the stump freely. He was unable to understand the case, and had consulted Professor Turner; but no explanation could be given, unless that there had perhaps been tension of the nerves relieved by the operation.

In reply to a question by the President, it was stated that the patient had feeling in the outer fingers.

(3.) TWO SPECIMENS OF HIP-JOINT DISEASE. In the first there was destruction and elongation of the head of the bone. As there was anchylosis, he first divided the femur below the trochanters, and then got the head of the bone out. In the second specimen there had been destruction of cartilage and caries. In both cases there was suppuration, but no external sinuses. He believed that the operation put the patients in a better way for recovery.

II. *Dr Wyllie* showed A VOLUME OF CHARTS he had introduced since he had taken charge of the scarlet fever wards in the Infirmary. On one side, the pulse, temperature, respirations, specific gravity and ounces of urine, number of motions, history of case, etc., could be recorded, and there was room on the back for detailed remarks. He believed it might be also useful in private practice, as giving shortly detailed accounts of cases.

III. *Mr Chiene* showed a new material which he had lately introduced for SPLINTS. The poroplastic splint commonly used was now six shillings and sixpence per lb., a price which interfered with its general use. After many experiments he had landed on a material used by wholesale hatters, and made of layers of calico and shellac. When cool it is stiff, but becomes quite pliable when heated before the fire or over a gas flame, so that it can be moulded to the desired shape. As yet he had used it of the thickness used by the wholesale hatter. It can, however, be made in any thickness. The price at present was two shillings and eightpence per lb.; but if introduced into use it would be made in greater quantity, and therefore cheaper still.

IV. *Dr Matthews Duncan* then read a paper on COMPLETE INVERSION OF THE UTERUS, which appears at page 769.

*The President* said the Society was much indebted to Dr Duncan for his interesting communication. The accident was one dangerous to life, and it was not yet quite decided what the best method of treatment was. The subject was one more suitable for those who attended on ladies; but it commended itself to surgeons too, as being partially a surgical operation. His own view was that the more recent the accident the more easily would the inverted uterus be replaced, just as in the case of prolapsus of the bowel. If, however, the uterus had been inverted for fourteen years, it hardly could be expected that it was possible to return it, as one of Dr Duncan's cases clearly showed.

*Dr Keiller*, who now took the chair, expressed the pleasure he had in acting as Vice-president, and was only sorry that he was unable to attend the meetings oftener. He, however, was fortunate in hearing Dr Duncan's paper. As an obstetrician he looked on it as valuable because of the extreme rarity of such cases; and Dr Duncan was right in considering himself fortunate, or perhaps unfortunate, in seeing so many cases in his own practice and that of others. So rare was the accident that many obstetricians of long experience had never seen it. In 200,000 cases at the Dublin Hospital there had been only one instance. In the Obstetrical Society, when the subject was brought before it, Sir J. Simpson, in reply to a member who said he had seen many such, stated that he had never seen a case of acute inversion. Dr Moir had seen one or two. He himself, a long time ago, had seen one case of acute inversion of the uterus with the placenta attached. The doubt was whether he should remove the placenta, and then reinvert. He removed the placenta, and then reinverted the uterus. Of his other cases, one was seen with the late Dr Dumbreck. It was a first delivery, and the woman had symptoms Dr Dumbreck did not understand. He was called in two days after, and found the inverted uterus in the vagina, and away from the pubes. As he had no chloroform, he went home for it, calling for Sir J. Simpson on his way. Simpson, however, was not at home. He with difficulty reinverted the uterus, adopting the plan of passing the fingers of one hand into the cupshaped depression above the pubes, and with the other pushing up from below. It took two hours to do it, but he succeeded at last, and the woman did well. His next case was in St Andrews, where Dr Archibald had delivered the woman some time before, but as some hæmorrhage ensued, she kept her bed. When called in consultation he found the uterus inverted, and as it had a fibroid tumour, the inversion was chronic. He pulled the tumour out, and removed it, with little bleeding, by means of the ecraseur. He then tried to reinvert the uterus, but failed, although tremendous force was used. He went back again with a special instrument, but again failed. The patient then came to Edinburgh, and was reported to the Obstetrical Society. He endeavoured to reinvert it, trying for days, and even weeks, with the caoutchouc bag, but utterly failed. The surface of the uterus could in this case be freely handled without causing any pain, only a little bleeding. Several operations were now proposed. Sir J. Simpson proposed to incise the body, but this was not done for fear of peritonitis. It was also proposed to incise cervix. The patient ultimately left for Dundee, and remained there, still ill from the hæmorrhage. She was alive yet, but with the uterus inverted. He had lately learned from her husband that she was now strong and could walk about. Menstruation had ceased, and the uterus was atrophying. No operation, therefore, seems needed. Another case happened at Stockton-on-Tees, and was interesting, as it caused a legal action on the part of the woman for the loss of her uterus. It occurred in the practice of Dr Watson there, who delivered the woman, and, according to her friends, did not use care enough. After delivery, Dr Watson found the uterus was inverted, retention of urine being one of the symptoms. He tried to reinvert it, and thought he had done so. The patient, however, was still uncomfortable, and therefore another surgeon was called in. He found the uterus still in the vagina, but failed to return it. He therefore got a third practitioner, who, in order to stop the hæmorrhage, removed the uterus by the ecraseur. The woman recovered, and then brought an action of damages for £1000 against Dr Watson for the loss of her uterus. Dr Watson accordingly asked him to give evidence. The trial lasted for two days, and he took up the apparatus used at the Maternity Hospital. In the witness-box he mentioned chronic cases where reinversion had been successful, and showed the caoutchouc bag in order to prove to the jury that the uterus ought not to have been excised. The result was that the poor girl lost her case and had to pay costs, Dr Watson getting off free. Dr Duncan's paper was a very important one, and with his views as to the causes of inversion he agreed. The cervix had little to do with it, but traction on the wall was important. He only wished now to mention an experiment he had performed. It was in a case of placenta praevia, where one of his students removed the placenta and tried to turn. As the woman was evidently dying, he extracted the child, and, with his one hand in the uterus, tried to invert it by pressure from above on the fundus. He utterly failed, although the uterus was not contracting. The common opinion, therefore, that inversion was caused by pulling on the cord, was wrong.

*Dr Charles Bell* regretted that he had been prevented from hearing the commencement of Dr Duncan's paper. Two points struck him, after coming into the room, on which he should like to have some information—the one was the size of the uterus which had been excised; the other was that Dr Duncan had said the cervix had nothing to do with inversion. He, however, believed, if it had nothing to do with the inversion, it had a great deal to do with preventing its reduction, by acting as the stricture in a strangulated hernia does. He believed that the *os uteri* generally contracts immediately after delivery, as shown by the difficulty in removing the membranes when the placenta is being contracted. They might indeed be left in the uterus, unless great care was used in removing them, because the *os* frequently contracts round them. This was a beneficial arrangement of Providence to prevent inversion, especially in tedious cases where the uterus gets exhausted, and the cervix remains dilated. When the child is born the latter contracts, and therefore the cervix has a good deal to do with preventing the reduction. The necessity of incision of the uterus proved that this was one great cause of the difficulty in reducing the inversion. This action of the *os* and cervix is further illustrated in his paper on hour-glass contraction, of which it is generally the cause.

*Dr Angus Macdonald*, in common with the members who had spoken, wished to express the pleasure he had in listening to a paper on a subject so interesting to the gynäkological practitioner. It was a rare lesion, and in proof of that he might mention that he had never in his experience in the New Town Dispensary seen acute inversion. This showed, firstly, that it was rare; and, secondly, that the young members of their profession who attended dispensary cases, not immaculate practitioners, do not drag excessively on the cord. Indeed, the plague in his life was being summoned to remove lazy placentæ, which he almost invariably found lying half out and half in the uterus. He had had the advantage of seeing one of Dr Duncan's cases, and had under his care at the present time a case of chronic inversion which had lasted for fifteen months. He could bear out to the full the points emphasized by Dr Duncan, and apparently not agreed to by Dr Charles Bell, viz., that the cervix uteri had little to do with inversion. It was fully dilated in his case, the cervix being fully traversable by the examining finger, the constriction being at the inner *os*. As to the treatment, he could also bear out the remarks on the difficulty of reposition. He had been unsuccessful in an attempt to reduce by taxis, although he had tried it with great force for an hour, the patient being deeply anæsthetized. He had employed Späth's method of gradual dilatation by guttapercha dilators. The difficulties, however, were very great, not so much on account of the amount of irritation and bleeding occasioned—for these, though considerable, could be got over—as from the impossibility of maintaining continuous tension in the elastic ball while in the vagina. It either lost its tenseness by continuous distension, leading to its tissue giving way and bulging at an unsupported point, or it was forced out of the vagina He felt sure that all interested in obstetrics would thank Dr Duncan for his extremely rare cases.

*Dr M. Duncan* had only to thank the members for the way in which they had received his paper. The remarks of the President and those of Dr Macdonald needed no reply, but his thanks, as they agreed with him. Dr Charles Bell's criticisms were founded on the assumed physiological fact that the *os* contracts immediately after the child is born. As this is not the case, the whole superstructure reared on it falls to the ground.

IV. *Mr Annandale* then read a paper entitled, NOTE ON ESMARCH'S OPERATION OF A PERMANENT CLOSURE OF THE JAWS, and showed two patients in illustration of his remarks. The paper appears at page 779 of this Journal.

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXXVI.—MEETING IV.

*Wednesday, 10th January* 1877.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Matthews Duncan* exhibited a recent specimen of SIREN FŒTUS, which had been presented to him by Dr Cran of Salford, Manchester. It was at or near the full time, and about 13½ inches long. No appearance of sexual apparatus externally. The limb reversed; the patella large and broad. Below the knee there were two inches of further limb ending in a rounded blunt point.

II. *Dr Underhill* exhibited a CELOSOME FŒTUS. The anterior wall of the abdomen was wanting, and the entire abdominal viscera were protruded, being only covered by the thin amniotic sheath of the umbilical cord. There were no indications of sex to be made out. At the bottom of the spine was a large hydrorachitic sac. The legs were turned round so as to lie along the back with the heels reaching to the head. The right foot was in a state of talipes varus, the left showed talipes valgus. The specimen had been sent to him by Dr J. R. Morrison, of Hartlepool. The fœtus appeared to be nearly at the full time. The head, upper extremities, and chest appeared to be well formed and natural. Dr Underhill intended to have a careful dissection made of the preparation, and would report the result to the Society.

*Dr Keiller* had met with a similar case in which the tumour presented, and was mistaken for the placenta.

III. *Dr Keiller* showed an ABORTION of six weeks.

IV. *Dr Matthews Duncan* read a paper on SOME OF THE RELATIONS OF THE FŒTAL HEAD IN RUPTURE OF THE PERINEUM AND INJURIES OF THE EXTERNAL GENITAL ORGANS (which appeared in our Journal for February).

*Dr Keiller* thought Dr Duncan's paper a most valuable one, and being based on personal observation and facts, none of the statements brought forward could be questioned. In practice he had found the difficulty and labour required to make such investigations as those of Dr Duncan. He was surprised at the frequency of perineal and other tears, yet had not hitherto considered that internal lacerations were so common as now, from Dr Duncan's paper, he believed them to be. It had often occurred to him that possibly these lacerations were more common and extensive in hospital than in private cases, on account of the frequency with which examinations were made in hospital as compared with private practice. This was an interesting point, and one he would like to have settled. In regard to Dr Duncan's last paper, he thought it also a very valuable one. In his experience, the greater the diameter of the fœtal head, the more probability of rupture. In forehead presentations he had frequently found rupture, and that generally central. Lacerations, he found, often began before the head had actually passed. Tears in the generative parts were very common in the lower animals during labour, more especially in the case of the cow. These were no doubt, in some cases, the result of the force used in effecting delivery. He could not close his remarks without referring to the extreme importance of these lacerations in their tendency to give rise to many post-partum pathological conditions.

*Dr Milne* asked a question, to which Dr Duncan replied in the negative, as to whether in any of his cases there had been after bad results. He was surprised at the frequency of these lacerations as detailed by Dr Duncan, and was inclined to think that they must be more common in hospital cases and from avoidable causes. He did not think that these lacerations were at all likely to produce fever or other morbid states after delivery, and was decidedly of opinion that the frequency of such cases was over-estimated. He did not think the forceps caused laceration, but, on the other hand, frequently prevented it.

*Dr Underhill* remarked that two important points were brought out most distinctly in Dr Duncan's first paper; first, that in primiparæ the fourchette frequently escaped laceration,—indeed it escaped in about half of Dr Duncan's cases. This was contrary to what is commonly taught in text-books and elsewhere, and served, he thought, to confute the argument of Dr Keiller and Dr Milne, that the large number of lacerations reported were due to officious interference on the part of house-surgeon or nurses; for less than was to be expected of ordinary perineal tears, such as are usually the only ones looked for, were found. Secondly, the great number of lacerations of other parts of the vulvo-vaginal opening; he was not prepared to find these so numerous and so extensive—in fact, hardly one of Dr Duncan's cases had escaped without tearing of some part or other of the orifice. He (Dr Underhill) thought partial central rupture, involving the vaginal mucous-membrane, but not reaching to the skin, were not at all uncommon, even when the fourchette remained entire. As regards the paper read to-day, he looked upon it as one of great value, and was much struck by the way in which the observations and measurements of Drs Duncan and Badin, begun with different objects and from different points of view, fitted into and were complementary of one another; together they formed a very valuable contribution to the mechanism of labour.

*Dr Macdonald* thought these tears of a minor degree quite as common in private practice as in hospital, but he feared that the true explanation of the apparent discrepancy was that they were often overlooked in the case of the private patient. Though he had never had an opportunity of seeing complete central rupture, he could bear out Dr Duncan in his statement regarding the occurrence of those tears beginning posteriorly and traversing the perineal tissues down to the skin, but not coming through it. Cases of that kind had frequently occurred in his experience. He thought that, during delivery, tears often occurred during the passage of the shoulders, and he wished Dr Duncan had treated of this. As to the relation of these cases to fever, septicæmia, etc., he had no doubt there was some important connexion.

*Professor Simpson* was not astonished at the frequency of these lacerations, as exact observations such as those of Dr Duncan and Schroeder had always showed them to be present. Many years ago, when investigating this subject, he had found that in all primiparous cases there was a rupture. He alluded to a case in which an extensive internal rupture occurred, a large flap of tissue being extruded without rupture of the perineum. In this case no tear could be felt before the head passed. In the Maternity Hospital he had lately had opportunity of verifying the results of Dr Duncan. Referring again to the frequency of rupture in primiparæ, he noticed that, in a paper by Dr Hunter on an Epidemic of Puerperal Fever in Linlithgow, it had been observed that the fever had specially attacked primiparæ. In multiparous cases in the Maternity he had found a good many internal fissures. Lacerations were more pronounced in proportion to the diameter of the fœtal head. He attached great importance to the passage of the shoulder as a cause of rupture. Lately, in a case at the Maternity in which forceps were used, there occurred only a slight fissure during the passage of the head, but when the shoulder came forward the tear extended back to the anus. He referred to the greater frequency of lacerations on the left than on the right side as brought out by Dr Duncan's paper, and wondered if this had anything to do with the position of the patient during delivery.

*Dr Duncan*, in reply, said that he did not attach importance to the opinion of practitioners who had not made deliberate and careful examination of the parts after delivery. He referred to the case he had related of a primipara in which he had no suspicion of rupture, and the patient complained of nothing whatever, and yet, on examination, he had found most extensive lacerations. As to the forceps, he would say, in answer to Dr Milne, that it had been proved in the Dresden Hospital that lacerations were more common in these cases; but he allowed that proof was yet required to demonstrate whether the lacerations in these cases were caused by the forceps, or the natural result of the conditions requiring the use of the forceps. He had little doubt there must be some connexion between the use of the forceps and the greater mortality in instrumental cases. He thanked the Fellows for the reception given to his paper.

# Part Fourth.

## PERISCOPE.

Dualistic Theory of Venereal Sores.—In criticising Mr Jonathan Hutchinson's remarks on this subject at the late meeting of the Pathological Society of London, the *American Journal of Skin and Venereal Diseases*, October 1876, says:—" We must express our disapproval of the statement so emphatically made at the commencement of the paper, in which Mr Hutchinson says—' I think we may say of dualism that it is dead, and that the now simpler creed, which attributes the soft chancre to contagion, with inflammatory products produced by syphilis, but not, as a rule, containing its germs, is the one which obtains general acceptance. We have thus in syphilis one malady and one virus.' We think this statement is, for various reasons, ill-judged. As dualism is not one of the distinct issues of his paper, he should not thus curtly allude to it and dismiss it, as by so doing he led the way to irrelevant discussion, and, at the same time, rendered himself liable to mislead those who are not familiar with the minute aspects of the question. Clinically, to-day, the fact that the hard sore always produces syphilis, and that the soft sore does not, but that it is simply local in its operation, is as generally received, and as susceptible of overwhelmingly convincing proof, as it was when it was so boldly advocated by Bassereau twenty-odd years ago."

Clinical Study of the Treatment of some Complications of Stricture of the Urethra. By E. Martin, from M. Guyon's Cases in the Necker Hospital, Paris.—In the treatment of simple strictures, M. Martin recommends wax bougies of the same calibre throughout. He dips the point in collodion to make it stiff, and to preserve any curve the operator may give the instrument. His object in using wax bougies is the same for which they were recommended long ago—namely, that they may take an impression of the strictured part of the canal. In difficult cases M. Guyon employs filiform instruments afterwards. Of 300 cases of stricture treated in the hospital, 24 are described by M. Martin as difficult, but only 3 of these ultimately required the knife. He very naturally, therefore, claims for the "gentle" system of treatment the credit of being very successful. In cases of stricture complicated with fistula, M. Martin reiterates the usual experience of surgeons, that most fistulæ heal up when the stricture is cured. Where this is prevented by induration of the tissues, he recommends perineal section or excision of the fistulous track. For the latter operation, the method of M. Voillemier is described, where by an oval incision a conical portion of the perineum is removed, having its apex at the internal or urethral opening of the fistula.—*Archives Générales de Médicine*, April and May 1875.

PROFESSOR DOLBEAU ON REDUCTION OF DISLOCATIONS OF THE HIP BY FLEXION AND ROTATION.—Professor Dolbeau characterizes this method as "de douceur," when contrasted with the old system of ropes and pulleys. He has employed manipulation successfully in 5 cases. He describes the *modus operandi* (procédé de Desprès) thus: 1. The thigh to be bent to a right angle with the body. 2. To be rotated inwards and outwards whilst extension is maintained. 3. The maintenance of this position relaxes the gluteal and triceps muscles, and prevents pain; and, 4, places the head of the bone in the best position for slipping into acetabulum during extension. 5. Very slight force is necessary if relaxation of the muscles is attended to. The main point is, therefore, the flexion of the thigh on the body; and Professor Dolbeau thinks that the method, slightly modified according to circumstances, is sufficient for the reduction of all cases of dislocation of the hip. He says that the object of his paper was to point out this.—*Bulletin Général de Thérapeutique,* March 1875.

WE quote the following from a note on the GURJON-OIL TREATMENT OF LEPROSY at Port Blair. The method was fully described in this Journal.

"Nine surgeons and two deputy-surgeons-general inspected the lepers at Port Blair, and all expressed their conviction that the remedy was a success. One of the latter, the late lamented Dr Duff, of the Madras Medical Service, remarked that it was more than a cure, almost a miracle. He said, to cure a leprous ulcer effectively was a marvel; much more the total eradication of other serious symptoms! One man had extensive ulceration on the dorsum of one of his hands, which never got well, and baffled all the hospital treatment for years, even carbolic-oil application in various degrees of dilution. At last it was thoroughly healed up by me with nothing but gurjon-oil in a few months, leaving behind a sound cicatrix. To my knowledge it remained so for a period of two years, all the while looking quite healthy and firm, and was a case frequently shown to visitors, whether medical or otherwise. On Dr Duff's visit in order to test the physical energies of the lepers, some of the men were made to run; they ran exceedingly fast, and afterwards stated that they were able even to go two miles at that pace. A few were made to take axes and mamooties, and strike at the trunks of trees, and dig the earth—remember the latter is like rock itself, so very hard—and they did so successfully. These men's hands were so benumbed previously, that they could not even grasp a spoon, a walking-stick, or a lotah, without letting them fall out of their hands. Most of the lepers would have been in their graves now had it not been for the gurjon-oil.

"There is a well situated at a distance of one hundred and fifty yards from the leper barrack, where the men assemble and bathe every morning. I used to be frequently present there myself. Gunga Ram, the subject of case No. 3 in Dr Dougall's report, whose past condition it would be needless to allude to here, as everything relative to him has been fully detailed already, is now capable, without the aid of another, to rub himself, pour water freely, and all the while firmly grasp the lotah between his fingers. So rapid has been his improvement after a few months' treatment, that, after the ablution is over, he cleans the brass vessel with sand, and gives it such a polish that nobody would believe his fingers were ever affected.

"Prior to the gurjon-oil remedy, the constitutions of lepers were so contaminated, that any little scratch or abrasion would take an ulcerative form, and chronic ulcers in particular were generally known to heal and break out again, whereas now once they have healed there is no return."—*Indian Medical Gazette,* October.

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## MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris, and of the Pharmaceutical Society of St Petersburgh; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

HYOSCYAMINE.—Dr J. Sydney Pearse has made some observations on the action of crystalline hyoscyamine, which is imported from Germany. The strength of the solution used by him was 1 grain in 2 drachms. Externally applied to the skin, it relieves pain. Painted on the skin in the neighbourhood of the eye, it does not affect the pupil, thus differing from atropia. Dropped on the conjunctiva, it acts similarly to atropia, the dilatation, however, not being so rapid; the application causes no uneasiness. When given hypodermically, one-sixtieth of a grain causes dryness of the skin, mouth, and pharynx, with thirst, flushing of the face, injection of the conjunctiva, dilatation of the pupil, and loss of power of accommodation of the eye. The pulse is increased in frequency, and its fulness is also increased. Headache and giddiness occur, passing to a semi-conscious condition, and sleep. The skin sometimes becomes hot and dry, and a rise of temperature to the extent of 1° in the hour takes place, when the full constitutional effects were produced. No irregularity of the heart's action was observed. Atropia increases the flow of urine, but hyoscyamia, if anything, diminishes it.—*Lancet*, 2d Sept. 1876.

Pulmont treated five adults affected with chorea, three of whom were pregnant, by means of hyoscyamia. The dose given was one-fortieth of a grain twice daily, gradually augmented to one-third of a grain. When symptoms of intoxication, dryness of the mouth, and dilatation of the pupils occurred, the administration of the drug was suspended, and it was noticed then that the choreic movements, which had undergone great diminution, augmented. The treatment proved very effective in chronic cases, the improvement usually setting in in eight or nine days.—*Bull. Gen. de Thér.*, 30th Aug. 1876.

URÆMIC CONVULSIONS TREATED BY MORPHIA.—Dr F. E. Potter records a case of uræmic convulsions treated with beneficial results by a subcutaneous injection of 12 minims of Magendie's solution of morphia. The result of this case, Dr Potter thinks, goes far to show that the danger to be apprehended from the use of morphia in uræmic convulsions has been much exaggerated. The effects of opium upon the cerebral circulation, whereby the comatose condition arising from a poisonous dose of that medicine is produced, in all probability differ widely from those which are caused by the non-elimination of the effete matter which in Bright's disease gives rise to uræmic convulsions; and though there is a strong analogy between the effects of the drug and of the poison, the above case tends to show the groundlessness of the fear that, by administering opium, we shall add fuel to the fire.—*Boston Med. and Surg. Journal*, No. 21, vol. 17.

MORPHIA IN INSANITY.—Dr M'Diarmid points out that morphia given by the mouth is liable to be altered and have its virtues impaired by the secretions and by the constituents of the food. If it escapes the action of the gastric juice, it has to traverse the liver cells, and even if the hepatic structures have no effect on it, it may affect them injuriously. Among other advantages of the hypodermic administration we can accurately apportion the dose, and we can administer the drug to patients who are unable or unwilling to swallow. The action of morphia, moreover, is much more speedy when given subcutaneously than when given by the mouth. Dr M'Diarmid has treated successfully by this method melancholia, acute mania, and the excitement of general paralysis. In continuance of the hypodermic treatment it is necessary steadily to increase the dose. Vomiting occasionally occurred, generally when the drug was administered in the morning. Strong coffee or combination with atropia were found to be the most successful means of obviating this symptom. Dr M'Diarmid advises that morphia should not be given hypodermically in cases of heart disease, or in persons exhibiting any symptom of serious brain disease, general paralytics excepted. In melancholic females he begins with one-ninth grain of the acetate once in twenty-four hours, and he never exceeds one-fourth grain. In melancholic males he generally begins with one-sixth grain once daily, and he seldom goes beyond one-fourth or one-third of a grain. In acute mania, one-fourth or one-third grain is given at first, and in recurrent mania one-third or one-fourth grain will suffice to procure quiet sleep. General paralytics require and bear the largest dose. It is rarely necessary to give the morphia hypodermically oftener than three or four times consecutively. He used in his experiments a solution of acetate of morphia in distilled water, acidulated with a few drops of glacial acetic acid, and having any excess of acid neutralized by liquor potassæ. The site of puncture was over the deltoid.—*Journal of Mental Science*, April 1876.

In the *Practitioner* for June is a paper by Dr Breward Neal on the use of opium and morphia in the treatment of certain forms of insanity. The paper does not admit of abridgment.

MORPHIA IN DELIRIUM TREMENS.—In the *Medical Times and Gazette*, 18th March, Dr Arthur Leared records a case in which, after failure with ordinary doses of opium, the patient was cured by five grains of morphia in two doses given subcutaneously. Taking into account the greater activity of the drug when so used, the quantity was probably equivalent to more than eight grains taken into the stomach. Dr Leared prefers the hypodermic method in such cases, on account of the greater rapidity of action of the remedy when so administered.

Mr Edward Nicholson (*Med. Times and Gazette*, 11th March) adopts the morphia treatment as recommended by Professor Roser of Marburg. This consists in the administration of energetic doses of morphia, commencing with one or two grains. Mr Nicholson's experience of the treatment is that it is safe, quick, and attended with a minimum of trouble. Mr Nicholson gives at once two grains of morphia, and in violent cases as much as three grains; this is repeated after two hours if no effect is apparent. A third dose, making a total of eight grains, within four hours, has sometimes been required. The patient generally falls asleep after the second dose, and awakes cured. Sometimes a further small dose of one grain may be required, but the patient is reasonable, and all trouble at an end.

MUSCARINE.—According to M. Alizon, subcutaneous injection in a frog of this substance caused cessation of the heart's action in diastole, the organ retaining its muscular irritability. Atropine prevents the production of this effect. In mammalia, muscarine first accelerates the heart's beats and then slackens them. In small doses the respiratory movements are at first increased in number, and then return to the normal standard. In medium doses the primary increase is followed by decrease, and poisonous doses cause progressive diminution to final stoppage. All these effects are produced after section of the pneumogastrics, and disappear by the use of atropine. Small doses of muscarine give rise to a slight rise in bodily temperature, which is not constant; medium doses induce a fall of from one to two degrees, followed by a return to the normal temperature; poisonous doses cause a very decided fall preceding death. Atropine raises the temperature. *Académie des Sciences*, 20th March.

PHYSOSTIGMINE.—Professor Laqueur of Strasburg suggests the employment of physostigmine in all cases of glaucoma simplex, especially when there is no abnormality in the iris or anterior chamber; also in all cases of glaucoma in which iridectomy has been performed without benefit; and in those cases of secondary glaucoma in which the movements of the iris are not impeded by anterior or posterior synechia. The action of physostigmine in diminishing abnormally increased tension probably depends upon its power of directly stimulating the smooth muscular fibres of the choroidal vessels. In a recent work, Harnack and Wilkouski have shown that physostigmine stimulates both smooth and striped muscular fibre independently of terminal nerve-filaments.—*Canada Med. and Surg. Jour.*, August.

CALABAR BEAN.—In the *British Med. Journal*, 28th October, Dr W. Monro records a case in which the application of an ointment of the strength of twenty grains to the ounce was efficacious in restoring the secretion of milk. The drug acts by dilating the peripheral bloodvessels.

PICROTOXINE.—M. Gubler obtained good results from subcutaneous injection of doses of one milligramme of picrotoxine in a case of labio-glosso-pharyngeal paralysis. At the points of injection, small hard indolent tumours formed which gradually disappeared. Dujardin-Beaumetz gives picrotoxine in epilepsy in doses of $\frac{1}{4}$ to 3 milligrammes; he did not find any good from it in paralysis agitans.—*Centralblatt*, No. 32.

SANTONIN IN EPILEPSY.—Mr J. H. Horne was led to administer santonin in two cases of epilepsy, thinking that the disease depended upon the presence of lumbricus in the intestine. In both cases the administration of the santonin was attended with marked benefit. Mr Horne asks—"May not the evident cerebral disorder set up by the administration of santonin be in some measure curative of epilepsy?"—*British Medical Journal*, 16th December.

SANTONIN POISONING.—In a recent communication to the Medical Society of the Lower Rhine at Bonn, Professor Binz recommended the administration of chloroform or ether, in cases of poisoning by santonin, together with purgatives and plenty of drink, to assist in the elimination of the drug. In poisoning by santonin, spasms come on, commencing in the face and descending to the extremities; the respiration is endangered; the seat of irritation is within the spheres of the nerves of the third up to the seventh pair.

SASSAFRAS.—In the *Cincinnati Lancet and Observer* for April, Dr A. W. T. Lyle has a paper in which he calls attention to the statement of Dr Thomson of Nashville concerning the antagonistic properties of sassafras to henbane and tobacco. Dr Lyle records a case of a child four years old who had eaten stramonium flowers, and showed symptoms of poisoning. After the administration of emetics, ten drops of oil of sassafras were given every half-hour until six doses had been taken. Consciousness then returned. A dose of castor-oil was given next day, and the child was free from all symptoms.—*New Remedies*, September.

SOLANUM.—From Dr Harley's observations it would appear that a quantity of the juice of this plant equal to six or even nine ounces of the fresh herb may be taken daily for many days without appreciable effect; that a quantity of the tincture equal to more than one and a half ounce of the ripe fruit, including the seeds, may be taken for a considerable time also without appreciable effect. Hence a decoction or infusion of the dried young branches of the plant (the part directed to be employed in the Pharmacopœia) is equally destitute of any active principle soluble in water; for if there were such, it would be contained in the expressed juice. These results are in accordance with those of Dunal and Fages, of Frank and of Garrod.—*St Thomas's Hosp. Reports*, vol. 6.

ACTION OF STRYCHNIA ON THE EYE.—Dr Hippel states that strychnia, in doses of from two to four milligrammes, produces the the following effects:—Increased peripheric sensibility for blue; temporary increase of visual power; more distinct perception of peripheric points, and lasting enlargement of the field of vision. He thinks that the effect on the optic nerve is the same as that attributed to the continuous electric currents on the other nerves.—*Bull. Gen. de Ther.*, 15th March.

STRYCHNIA IN ASCITES.—Dr M'Kie records two cases of ascites in which considerable improvement took place under the use of strychnia.—*Amer. Jl. Med. Sci.*, April.

STRYCHNIA IN ACUTE NEUROSES.—Dr L. B. Edmunds extols the use of strychnia in acute neuroses due to tobacco, especially in cases marked by nervousness, muscular irritability, tremor, etc.—*Detroit Review of Medicine*, July.

STRYCHNIA AND WOURARU IN RABIES.—In the *American Journal of Medical Science*, July, is a long article from the pen of Dr B. A. Watson in a supposed case of canine rabies treated successfully with strychnia and wouraru. The paper does not admit of abridgment.

CHLORAL HYDRATE IN CHOREA.—Drs Goltz and Auger give the details of an obstinate case of chorea minor, in which eserin and subcutaneous injections of morphia had been tried without success, and in which they determined to try clysters of chloral hydrate. They state that this drug cannot be well given by subcutaneous injection, on account of its tendency to produce local irritation. They commenced the treatment with an enema twice daily, containing 45 grains of chloral; only slight improvement occurred at first, and they increased the dose to 60 grains twice a day. Under this treatment complete recovery took place in fifteen days, and was not followed by any relapse.—*Practitioner*, Dec. 1876.

CHLORAL IN TETANUS.—At the French Academy of Sciences, 22d May, M. Oré reported a case of traumatic tetanus, cured by intravenous injection of chloral. The injections were made from 23d June up to 26th July 1875, and the total quantity of chloral injected was 56 grammes.

In *Practitioner* for December last, Mr J. H. Salter records a case of acute traumatic tetanus treated successfully with chloral hypodermically injected. The treatment consisted in wearing out the acute character of the disease by the continued exhibition of the drug; neutralizing the tetanic poison, so to speak, as fast as it was secreted; or, in other words, de-polarizing the nervous centres excessively charged by the morbid processes of the disease. The chloral was generally injected in doses of 5 grains dissolved in 5 minims of water.

Drs Laurens and Bresson report two cases of traumatic tetanus in which the treatment adopted consisted in the administration of chloral. In one case, the patient was kept under the influence of the drug for thirteen days, with a successful result.—*Bull. et Memoires de la Soc. de Chir.*, t. ii. 1876.

CHLORAL IN PUERPERAL CONVULSIONS.—Dr Chouppe believes that of all means which we possess, hydrate of chloral is the most reliable for treating this disease. In twelve cases it proved successful. He advises its administration even before the disease becomes confirmed, whenever the woman exhibiting albuminuria and œdema complains of headache, singing in the ears, hallucinations of vision, restlessness, cramps in the limbs, etc. When there is trismus present, it should be given in enemata. The dose must be according to the tolerance of the patient and the severity of the paroxysms, but it is a necessity to commence with a pretty strong one to make a powerful and quick impression. After a calm has been obtained, smaller doses may be given during the next twenty-four hours or so; but if the attacks recur, large doses must again be resorted to until the paroxysms have completely ceased. In an enema, we may begin with 30 grains, repeating this at the end of ten minutes. In all cases, it is of importance to get at least 60 grains rapidly taken, and to prolong the use of the chloral for a tolerably long time after the cessation of the convulsions.—*Gazette Méd.*, 5th Aug.

CHLORAL IN INFANTILE CONVULSIONS.—Lowenstamm refers to numerous cases of infantile convulsions successfully treated with chloral. He gives details of one case. Two grains of chloral hydrate were given every hour with the effect of diminishing the intensity and frequency of the convulsions.—*Medicinische-Chirurgisches Centralblatt*, No. 38, 1876.

ANÆSTHESIA BY INTRAVENOUS INJECTION OF CHLORAL.—The following are the conclusions arrived at by Sigs. Tizzoni and Fogliata as the result of forty experiments:—1. Chloral injected into the veins does not act as a true anæsthetic, but is simply a strong hypnotic. The sensibility of the skin is abolished only when a very strong dose is given, whilst the cornea scarcely ever loses its sensibility. 2. Production of anæsthesia by intravenous injection of chloral is very dangerous. The action of the poison varies considerably in different individuals, and cannot be arrested if excessive. Phlebitis is easily induced. 3. The amount of water injected is not free from injurious effects apart from the danger of injecting air. Chloral is a cardiac poison, and when injected in toxic doses arrests the heart in diastole, whilst, if locally applied, it causes tetanic contraction. 4. The subcutaneous injection of chloral is not advisable, as it is slowly absorbed, and is not unfrequently followed by gangrenous abscesses. 5. The best antidote to chloral is the cold douche to the head and back. The injection of strychnia, quinine, atropine, or wourara is injurious. The inversion of the patient is also useless.—*Rivista Clin. di Bolog.*, Fas. 12; and *Pract.*, Sept. 1876.

EFFECT OF CHLORAL ON THE KIDNEYS.—Mr Charles Orton accredits chloral hydrate with the power of inducing congestion of the kidneys. In two cases a post-mortem examination revealed an intense state of congestion of these organs, death having occurred after taking large doses of chloral. Mr Orton's experiments on animals have convinced him that congestion of the kidneys almost invariably follows the use of chloral.—*British Medical Journal*, 9th December.

PAIN PRODUCED BY CHLORAL.—In the *British Medical Journal* for 2d September, Mr Herbert M. Morgan states that in several cases chloral-hydrate, in ordinary doses, generally, when continued for several days, caused a feeling of pain all over the body, often so severe as to make the patient beg for relief. In each case no relief was obtained until the chloral was discontinued. It seems to be due to a general hyperæsthesia of the cutaneous nerves, but sometimes localized in one particular spot. Tincture of gelseminum relieves the pain sooner than other remedies.

ADMINISTRATION OF CHLORAL.—The following formula is an agreeable one for the administration of chloral:—Finely-pounded sugar, 100 parts; chloral-hydrate, 5 parts; water, 15 parts. Dissolve the chloral in the water, and triturate with the sugar in a mortar. An aromatic flavour is then obtained by the addition of the artificial essence of pine-apple, or the essence of peppermint.—*Medical Examiner*, 21st September 1876.

LOCAL ACTION OF CHLORAL.—Dr Guido Tezzoni observes that the first effect of solution of chloral-hydrate to parts deprived of epidermis and epithelium is a transient irritation, which is followed by an anæsthetic action. The irritant action favours the development of cellulo-vascular granulations, and accelerates cicatrization. The irritant action having ceased, there is in a few hours an arrest of the inflammatory processes, due apparently to retardation of the circulation and constriction of the capillaries, as well as to the minute coagula and embola which are found in the vessels, and which cut off or diminish the local supply of blood. Tezzoni advocates the use of chloral in surgery, on account of its properties as a disinfectant, antiseptic, antiputrescent, and antifermentative. He has treated many cases of acute and chronic urethritis with injections of chloral-hydrate in the manner recommended by Parona, and has found the result to be rapid cessation of the discharge and cure of the disease without complications.—*L'Independente*, 6th November 1876.

# Part Fifth.

## MEDICAL NEWS.

SOCIÉTÉ CLINIQUE DE PARIS.—On the 19th January, a certain number of Members of the Academy of Medicine, Professors of the Faculté, Hospital Physicians and Surgeons, and "Internes des Hôpitaux de Paris," met at the residence of Dr Bottentuit; and founded a new medical society, to be called the "Société Clinique de Paris." The objects of the Society are the study of diseases in relation to their pathogenesis, organic lesions, semeiology, and treatment, under the inspiration of the great traditions of Medicine, and in strict accordance with contemporary methods of investigation. The Society is to consist of 20 honorary members, 100 ordinary members, and an unlimited number of corresponding members. Members of all classes have a right to take part in the scientific discussions; but only the ordinary members [*membres titulaires*] have a right to vote and take part in administrative business. The number of ordinary members is already completed. No new ordinary members are to be appointed till there are three vacancies; and the list of candidates must be one at least in excess of the number of vacancies before any election is proceeded with. Meetings are to be held at the Mairie de l'Elysée on the first and third Wednesday of each month, at 8 P.M. The first meeting was held on Wednesday, 21st February, when the office-bearers were elected, after which the President (Dr Michel Péter) delivered an excellent inaugural address, which appears *in extenso* in *La France Médicale* of the 24th February, in which journal the official reports of the proceedings are to be regularly published. The following is a list of the office-bearers. *Honorary President*, Dr Barth; *President*, Dr Péter; *Vice-Presidents*, Drs Bucquoy and Le Dentu; *General Secretary*, Dr Dieulafoy; *Secretaries for the Meetings*, Drs Labadie-Lagrave and Huchart; *Treasurer*, Dr Carrière; *Librarian*, Dr Krishaber.

This new institution is evidently organized for the purpose of advancing the science of Medicine by clinical work performed with energy and industry. The ordinary members must either be Doctors of Medicine of Paris or "Internes des Hôpitaux;" and it is, we believe, intended that about a fourth of the ordinary members belong to the latter class, which always includes the most talented of the rising generation of French clinicians.

LIST OF INDIAN MEDICAL CANDIDATES who were successful at both the London and Netley Examinations, having passed through a course of instruction at the Army Medical School, Netley, February 1877.

| | No. of Marks. | | No. of Marks |
|---|---|---|---|
| *1. Moorhead, J. | 5638 | 7. Eaton, G. B. | 4525 |
| 2. Beatson, C. H. | 5393 | 8. Eyre, M. S. | 4505 |
| 3. Owen, C. W. | 5060 | 9. Channer, O. H. | 4490 |
| 4. Griffiths, G. S. | 5000 | 10. Young, E. W. | 4465 |
| 5. Grant, P. M. | 4966 | 11. M'Calman, H. | 4350 |
| 6. Lee, W. A. | 4934 | 12. Ross, D. R. | 4308 |

* Gained the Herbert Prize.

Mr JARDINE MURRAY has been appointed Honorary Consulting Surgeon of the Brighton and Sussex Eye Infirmary.

ENDOWMENT OF A DEMONSTRATORSHIP OF PHYSIOLOGY IN THE UNIVERSITY OF GLASGOW.—We have much pleasure in announcing that Dr Henry Muirhead of Bushyhill, Cambuslang, has generously offered to the University of Glasgow the sum of £2100 as an endowment of a Demonstrator of Physiology in connection with the Chair of Institutes of Medicine. The object of the endowment is the promotion of medical science by the training of young men of suitable capacity to become teachers and investigators of physiology; and as this training is best attained by actual work in the laboratory and by practical experience in the art of teaching, the demonstrator will be regarded as a teaching assistant to the professor, while he will also be encouraged to pursue independent original investigation, and will be aided by the use of all the laboratory appliances. The University possesses already a considerable number of bursaries and scholarships, which are, no doubt, of great practical use as encouragements to learning; but the foundation of Dr Muirhead, and the kindred foundations in the departments of engineering and natural philosophy, have this peculiar advantage, that they constitute not merely a reward for the past attainments of the student, but an incentive to the prosecution of his studies, and a provision for efficient work in the future. There are other branches of science in connection with which endowments for the advancement of research and the training of teachers would be equally useful; and it is to be hoped that the munificent example of Dr Muirhead will be followed by many other foundations of a similar kind.

MANY of our readers may have heard of the gigantic undertaking at present being carried out at the Library of the Surgeon-General's Office of the U.S. Army—the publication of a Catalogue Raisonnée, which aims at presenting to the profession a perfect bibliographical record of the medical literature of the world. No one who knows the talents, zeal, and untiring industry of the Editor, Dr J. Billings, will doubt that what can be done he will do. Many old theses, pamphlets, and reprints are still doubtless undiscovered even by him; and any assistance in unearthing such waifs and strays will be gladly welcomed by him and his Government, whose liberal and far-sighted policy in inaugurating this great undertaking cannot be overestimated.

The Publishers of this Journal will gladly receive and forward any books, pamphlets, theses, or reprints of papers other than those which have appeared in this Journal.

WE have received an interesting paper by Dr A. AMUSSAT ON THE USE OF THE WHALEBONE CONDUCTOR IN THE TREATMENT OF RETENTION OF URINE, etc.—The illustrations are beautifully executed, and show very clearly the ingenious adaptations Dr A. Amussat has devised of the whalebone conductor to bougies, catheters, and lithotrites. Perhaps the most ingenious employment of the filiform conductor is its being made to screw into an aperture in the point of an ordinary metallic catheter. This adaptation may make the instrument very useful in cases of difficult stricture.

## OBITUARY.

Sir William Fergusson, Bart, F.R.C.S.—This distinguished surgeon died at his residence in London on Saturday evening, the 10th of February 1877. He had nearly completed his sixty-ninth year, having been born at Prestonpans, East Lothian, on the 20th of March 1808. For many months, the state of his health, which during life had been uniformly robust, occasioned much anxiety to his family and friends; he rallied, to a certain extent, in the course of last autumn, which was spent, as usual, at his country seat, Spittlehaugh, Peeblesshire; but on his return to London the malady reasserted its power, and the fatal event took no one by surprise. Sir William was so identified with the Edinburgh School of Surgery—and, indeed, he always wished it himself—that we gladly linger for a little over his bright career, and endeavour to make his example a profitable one to those who may desire to emulate him.

Having acquired the ordinary elements of a sound education, first at the Grammar School of Lochmaben, and then at the High School of Edinburgh, he entered the University, and after a few months spent in a lawyer's office, began the study of anatomy, at the early age of seventeen, under Dr Robert Knox, the colleague, and ultimately the successor, of a no less famous man—Dr John Barclay. "Contrary to the advice of his friends (as Dr Struthers tells us in his interesting sketch of the Anatomical School), Barclay appeared to give the introductory lecture of session 1825–26; but his memory had failed, and it was his last appearance in the lecture-room." Young Fergusson was probably present on that occasion; but, be that as it may, he attended the lectures of Knox from the beginning, and, before another year had elapsed, became his trusted assistant. Our own acquaintance with him dates from the winter session of 1828–29, a period of much popular excitement, and much anxiety to Knox and his coadjutors, in connexion with the story of Burke and Hare, the West Port murderers. William Fergusson, then a young man—barely twenty-one—was demonstrator of anatomy. Along with several others, most of whom are now gone, he passed through the trying ordeal unscathed; and we cannot doubt that the fibres of his moral nature were thereby strengthened for the arduous work and discipline which awaited him in succeeding years. At that time, and long after, he laboured, almost lived, in the dissecting-rooms, performing all the duties of demonstrator in the most efficient manner. We retain a vivid remembrance of his appearance. Tall, lithe, handsome, with an animated, cheery countenance, a bright eye and manly bearing, he was always genial, ready to help, and patient in instructing the ignorant and uninitiated, most of whom were very little younger than himself, some considerably older. At a period somewhat later, while

John Reid, afterwards the distinguished physiologist, was taking a large share of the tutorial work of the dissecting-room, Fergusson might be seen assiduously occupied in displaying the arteries of a foot, or hand, or head, even to their smaller ramifications, by carefully removing with his knife and forceps all the surrounding soft textures. Of course, his purpose in so doing was, less to display the mere vessels, than to perfect his own dexterity in the use of his instruments. In these days also, we remember, he practised a good deal on the violin, not merely for the sake of the music, but mainly to increase the development and agility of the muscles of his hand and fingers. In short, he took remarkable pains with himself, in anticipation of his future occupations as an operating surgeon, excellence in which he had resolutely set before his mind. These are no afterthoughts; they were impressed upon us at the time. It may be mentioned in passing, that he had early displayed an unusual aptitude for mechanics, and had by this time surprised and gratified his friend Dr Knox, by presenting him with a dissecting-case entirely constructed and fitted up by his own hands. During his connexion with the dissecting-rooms, which we have said lasted for some years, he was in the way of giving very instructive demonstrations of regional anatomy, with special reference to the practice of surgery. This was a new thing in those days, and very acceptable to many of the students, giving, we believe, a decided stimulus to not a few men who afterwards distinguished themselves in that branch of the profession. We can see also how very useful such courses of instruction must have been to the teacher himself, by laying a solid, trustworthy foundation for his future eminence as an operator. Other means were not neglected. He acted, for one season at least, as class-assistant to Mr Turner, the Professor of Systematic Surgery, an able, well-informed, and learned man, whose mental and bodily constitution, however, did not qualify him for operative surgery. He took every opportunity, also, of witnessing the practice and brilliant operations of Robert Liston and John Lizars, who were at that time colleagues in the Royal Infirmary. The rising sun of James Syme was already above the horizon; but as he was not connected until 1833 with that institution, and professional cliques and rivalries ran high in those days, we are by no means sure that, until some years later, he exercised any material influence over Fergusson's education. In a certain sense, this was completed in 1829, when, having previously obtained his diploma, he was admitted a Fellow of the Edinburgh Royal College of Surgeons. His probationary essay on that occasion was entitled "The Arch of the Aorta," and was a very creditable piece of descriptive anatomy, with surgical references.

We may here pause for a little to consider the character and circumstances of the period in which he entered on the practice of his profession. He has, himself, in the first of his lectures on the

"Progress of Anatomy and Surgery during the Present Century," delivered before the Royal College of Surgeons of England in 1864 and 1865, and published in 1867, materially aided us in doing so, by furnishing us with his own personal impressions. These, on the whole, are just and accurate, and display, we think, a liberality and cordiality of mind quite characteristic of the lecturer. He is of opinion that, for some years previous to the year 1825, when his own studies commenced, "there was a period of calm such as had not been seen for many years, and such as none of the present generation have seen. There was nothing new in British surgery, and little from abroad to attract special attention."

"The brilliancy of John Bell had, in the early part of the century, given great *éclat* to the School, and the solid worth of Benjamin Bell (who died in 1806) had given a high character to Edinburgh surgery." "But," he goes on to say, "the field of surgical practice, at the period referred to, was held by gentlemen of high social and professional stamp, who, however, were neither professed teachers nor long-experienced hospital surgeons." It is quite true that the men who then excelled as operating surgeons—we refer chiefly to George Bell and William Newbigging—were not professed teachers; but both of them held their position, from first to last, for a long term of years in the Royal Infirmary, and were eminent operators —the former especially, who was very extensively consulted and employed throughout Scotland and the northern counties of England. Several other gentlemen mentioned by Fergusson were writers or translators of surgical books, but as practical surgeons they were not conspicuous. Even Mr James Russell, the first Professor of Clinical Surgery in the University, although the author of an original work on Necrosis, still referred to, was, as Mr Fergusson says, by no means prominent as a surgeon. By a curious anomaly, he had no wards of his own in the Royal Infirmary during the twenty-seven years in which he held the Professorship. He remarks, therefore, very justly, "that there seemed little hope for a continuance of the great reputation of the Edinburgh School, when suddenly there appeared on the scene three men, John Lizars, Robert Liston, and James Syme, whose labours have added substantially to the renown of the Scotch School, and whose names will be imperishably associated with the history of British surgery."

With these men preoccupying the comparatively limited field of the Northern Metropolis at that time, it will be seen that the prospects before our young surgeon were not very bright. But he did his best, and bided his time. In 1834, Liston left Edinburgh for London; and six years afterwards, in 1840, on the retirement of Mr James Arnot from the Chair of Surgery in King's College, Fergusson, who was known as a young surgeon of great promise, was nominated his successor both in the College and in the Hospital associated with it. He was then only thirty-two; but he rapidly attained distinction, and those more substantial benefits which ordinarily follow in its train. Liston died towards the close of 1847, and Fergusson was prepared to occupy his place in public estimation as a brilliant and successful operator. It is a popular mistake that either of these men operated for operating-sake; they regarded the necessity for employing the knife in many cases as an opprobrium, and both of them, but especially he whose professional life we are trying to sketch, were eminently conservative as surgeons. The volume of lectures, already referred to, furnishes a very good view of Fergusson's contributions to the advancement of surgery, and, at the same time, a favourable specimen of his style as a writer. We shall refrain from entering into a review of its contents, having done so at some length on a previous occasion, shortly after it was published. The concluding sentence of that review may be quoted, as it probably expresses what is still the general feeling towards the lamented author:—" On this side of the Tweed, it affords pleasure to find that the book is full of affectionate remembrance of his old school, which could not have eenb shown more gracefully than by the dedication of the work to his old friend of forty years' standing, Dr John Burt, late President of the Royal College of Surgeons of Edinburgh."

The work just referred to, and his " Manual of Practical Surgery," which has passed through five editions, and been translated into various European languages, indicate very fairly the author's peculiar powers and qualifications. They exhibit the surgery which Fergusson both taught as a Professor and practised with consummate skill and dexterity. But it may be said, without disparagement, that they exhibit neither the original inventive genius, nor the clear, accurate, incisive composition which characterized his great contemporary, James Syme. The former practised the art of surgery as he found it, with admirable skill and success, but did not originate many important innovations—always excepting, of course, his successful working out of the subject of hare-lip and cleft palate, his improved instruments for lithotrity, and his contributions to conservative surgery: the latter gave himself to the elucidation of sound principles in surgery, and introduced many valuable improvements in practice, including new operative procedures, with which his name will be always identified.

Mr Fergusson's distinguished merits as a surgeon were rapidly recognised in London, and it became apparent that the important position which he had obtained so unexpectedly had an occupant every way worthy. We have no belief in the common notion that a fortunate start in any man's career is all that is needed for ultimate success. The man must be equal to the occasion, otherwise his failure will be proportionally conspicuous. A remark of the late Sir William Lawrence deserves being pondered:—" It is less difficult to find situations for men, than men for situations." King's College and Hospital were fortunate in finding William

Fergusson; and the young Scotchman came very rapidly to the front, through sheer excellence and superiority, independently of any adventitious circumstances and notwithstanding able and accomplished competitors. Honours were conferred upon him in due course. At the death of Mr Aston Key in 1849, he was chosen Surgeon in Ordinary to H.R.H. the late Prince Consort. He became Surgeon-Extraordinary to the Queen in 1855, and Sergeant-Surgeon in 1867. A baronetcy had been conferred upon him in the previous year, "in consideration of distinguished merit and eminence as a surgeon." The following note to a friend in Edinburgh, who had written to him on the occasion, gives a passing glimpse of the character and right feelings of the man :—" Your kind note of congratulation gave me much satisfaction, for it recalled the memories of bygone times, and made me think of those anxious yet happy days when I was laying the foundation for all the success which has befallen me. I have, I assure you, great pride in considering myself of the Edinburgh School; and it is gratifying to think that I have many friends in the good old town, who, like yourself, are pleased with my recent elevation. Like many, I fear that I have thought more perhaps of human agencies than of the Supreme Ruler of all; but I am sure that I have every reason to thank God for a large share of prosperity and happiness throughout life." These words will recall to some those pleasant annual occasions on which Sir William used to dine with an association of professional brethren on the eve of his departure from Scotland to resume the labours of a new winter in London. He evidently enjoyed these social meetings with his friends—some of them old contemporaries; and his speech in returning thanks when his health was proposed, always gave evidence of his tender remembrance of days long gone by, and of his continuing a loyal son of his Edinburgh Alma Mater.

It may be gathered from what we have already said, that Sir William Fergusson, while possessing all the essentials of a great surgeon—anatomical knowledge, familiarity with disease and injury, clearness and quickness of perception—both mental and bodily—coolness, courage, physical strength, manual dexterity—had other qualities which made him a favourite with his friends, his patients, and the public at large. Although ready to assert and substantiate his own rights upon needful occasion, he was one of those large-hearted men "who live and let live;" who are generous and friendly towards their rivals, ready to give honour where honour is due, and not oblivious of former friends in the midst of worldly prosperity and success.

The portrait painted by Lehmann in 1873, and presented to the Royal College of Surgeons of England by a number of his friends and admirers, perpetuates the deceased very successfully in a characteristic attitude. A replica in half-length hangs in the hall of the Edinburgh College of Surgeons, surrounded by many professional worthies of former days; and an excellent engraving executed by Joubert leaves nothing to be desired.

His mortal remains rest in the quiet burying-place of West Linton, not far from his country residence, in the county of Peebles.

We cannot close this imperfect *In Memoriam* more appropriately than by adopting the eloquent and graceful words of Sir James Paget, in his recent Hunterian oration in the theatre of the Royal College of Surgeons, London:—

"Such was Hunter's great work in surgery, and this it is which still abides, for, since the time of Hunter, science has never yet been absent in the teaching of our schools. Since his time the teaching of practical surgery has never been divorced from the study of biology. They have been cultivated in various degrees in various schools by different men, but both have held their ground, and both will still increase and grow. Yes, both will increase, though the great master of art, the greatest practical surgeon of our time, is gone. Men will no longer watch those eyes that were so keen; nor try to imitate those hands that were so strong, and yet so sensitive, so swift and light; nor wonder at that keen and clear perception, the prompt invention, the perfect calmness in the greatest difficulties. All these are gone, and with them are gone those things that endeared him to us still more—the warm heart, the friendliness, the generous rivalry, and the social grace. These are gone; but Fergusson's lessons will still remain amongst us, and among them will be this, that every man according to his ability should have both art and science, should work with both as with both hands, as if with one mind and one design." "*Pulchrum est laudari a laudato.*"

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Death of Dr Fettes.—We are called upon this week to announce the death of this gentleman, which took place on the 22d inst., at the ripe age of 80 years. Dr Fettes was for considerably more than half a century the leading medical practitioner in the Mearns, and during this extended period enjoyed a practice as extensive as his powerful physical frame was able to meet. In addition to the amount of regard and confidence which his devotion to professional work gained for him among his numerous patients, the Doctor's social position in the country was always such as his self-denying labours and gentlemanly deportment entitled him to. Dr Fettes's diploma of the Royal College of Surgeons of Edinburgh is of a date as remote as 1818, at which time he settled in Laurencekirk. He was Parochial Medical Officer of the parishes of Laurencekirk and Garvock, from the time the present Poor Law came into operation until two or three years ago. He was also a Justice of the Peace for the county of Kincardine. Few who are old enough can fail to remember the handsome upright figure of the Doctor, mounted, as he always was, on his powerful gray horse, and encountered at all seasons all kinds of weather, and at all hours, in the western districts of the county. His devotion to his patients was unselfish and unremitting to a degree unusual, even in a profession proverbial for generosity. It was no unusual thing for him when making a late visit to a patient, the cause of anxiety, to borrow a pair of slippers, have his horse carefully put up and attended to, and pass the entire night in the house of the invalid, after having, during the winter's day, spent twelve or fourteen hours in the saddle; and, after an early breakfast, left, to pass such another laborious and exhausting day. The event which has just taken place, will afford opportunity for more than one mother to exclaim, "My best friend is gone! he brought all my bairns into the world, and he was never paid for one of them." Another of the few remaining links, binding the medical practitioners of the present day to those who occupied their places in the early part of the present century, is broken. Dr Fettes, who was twice married, had a large family by his first wife, who all predeceased him except his eldest daughter, who is married to Mr Alexander Bent of Haulkerton. The untimely death of his son, Dr Charles Fettes, of H.M. Indian Army, was a sorrow which the Doctor was long in getting over—probably he never did so entirely. For three or four years his memory has been so much impaired that he found it necessary to abandon professional work; but during the very protracted period already alluded to, no man ever did more work, or performed it more honestly.—*Montrose, Arbroath, and Brechin Review*, Jan. 26.

## CORRESPONDENCE.

*To the Editor of the Edinburgh Medical Journal.*

### The Astringent Action of Salicylic Acid.

Musselburgh, 1*st February* 1877.

Sir,—In the February number of the *Edinburgh Medical Journal* there appears an interesting paper by Mr R. Kirk, M.B., "On the External Use of Salicylate of Iron." In beginning his subject, he has unfortunately made a misstatement, which is so misleading that I ask your kind permission to point it out. He says, "Salicylic acid and its compounds had the disadvantage of being in no way astringent." This is entirely wrong. I have pointed out in my paper in the November number of the *Journal*, entitled "Notes on Salicylic Acid," that it was interesting to notice the close chemical relations which gallic acid bore to salicylic acid, as it had powerful astringent properties. I also mentioned the fact that, where pressure and cold failed, I arrested hæmorrhage by the powder of salicylic acid. My notes on this point were of a very paltry nature indeed, but I have the testimony of *L'Union Médicale* of 1875 to vouch for the fact that it has been used successfully in post-partum hæmorrhage and uterine catarrh. I may also mention that I have used it with glycerine as a topical application in quinsy, and with complete success in arresting the inflammation and restoring the tonsils to their normal condition. As pointed out in the February number of the *Journal* by Dr Hunter, it is a most powerful astringent on the bowels in typhoid fever, and I had also drawn attention to this in my notes of my experiments. In conclusion, I have to state that Mr Kirk's statement is quite opposed to the real facts, and is deserving of correction.—I am, Sir, yours truly,

John A. Erskine Stuart,
L.R.C.S. Edin.

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# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*The Diagnosis of General Paralysis of the Insane.* By J. BATTY TUKE, M.D.

(*Read before the Medico-Chirurgical Society of Edinburgh, 7th February.*)

THE experience derived from the observation of several cases which have come under my observation within the last few years has led me to doubt whether the diagnosis of general paralysis is such an easy matter as it is represented to be in most works on psychiatric medicine. We find Bucknill and Tuke in their Manual stating, "The diagnosis of general paralysis is of the most facile sort after practical knowledge of the disease has been acquired;" and Blandford says, "You will have to distinguish general paralysis, on the one hand, from less formidable varieties of insanity, on the other from certain other affections which, we are told, may be confounded with it. But these are rare, and the diagnosis is not difficult."—(*Insanity and its Treatment*, p. 276.) The same author alludes to cases of spurious or *pseudo general paralysis*, the symptoms and course of which do not present the usual appearances, and which by one observer may be called general paralysis, while another would deny them the right to the title. In the cases I purpose speaking of, all or almost all the well-known symptoms were present; their peculiarity lay in the very material fact, that they did not run the ordinary course of the disease.

In the summer of 1873, A. B., aged 43, was transferred to my care from a public asylum, in which he had been placed at the instance of the police. He had been under restraint for twenty-four hours prior to my seeing him, but during that time the medical superintendent had formed a guarded diagnosis of general paralysis—an opinion in which I coincided. The general health was fair; tongue furred; morning temperature, 98·6°; evening, 99°. The motor powers were unaffected, except that there was a considerable degree of stiffness and quivering of the upper lip, tremulous and jerky action of the tongue, and a hesitating slurred articulation. The pupils were equal, regular in outline, and responded freely to the stimulus of light. It was stated there was no hereditary predisposition, that the patient was in every respect a steady man, and had had no previous attack of insanity. It was subsequently discovered that all these statements were incorrect. The mental phenomena were as follows:—General restlessness and excitement, incessant rambling talk, chiefly on religious matters and the splendid prospects of his future life, self-complacency and satisfaction with everything around him; memory of recent events was very defective, *e.g.*, he said he had been under restraint for ten days, the fact being he had been for only two. Within three weeks A. B. developed violent mania, accompanied by very extravagant delusions, such as that he was the Holy Ghost, was possessed of immense wealth, was the King of Scotland, demanding his liberty in order to go to Holyrood to be crowned, that he was the Grand Master of the Good Templars, and finally that he was the Messiah. Coincidently with these physical symptoms the impairment of speech became more pronounced, and locomotion affected; his gait became straddling, and his walk slow and uncertain. The pupils were never affected. A. B. remained under my observation for three months, when it was found necessary for family reasons to place him in another asylum. I granted a certificate that he was labouring under general paralysis of the insane, and expressed an opinion to his relations that the case was hopeless. In his new residence the same train of symptoms persisted for some months, when he somewhat suddenly recovered, and till quite lately continued sane. Till within a short time of his recovery, the medical superintendent, under whose care he was, regarded the case as one of general paralysis. It was subsequently discovered that in A. B.'s family there was a distinct history of insanity, that he had seven years previously suffered from a precisely similar attack, which followed, as had the one I have just described, on a series of deep debauches. Since commencing to write this paper I have been informed that A. B. is again under treatment, with symptoms not so closely resembling general paralysis as those already mentioned.

In this case, I think it will be admitted, all the symptoms of the disease were present, except those connected with changes in the retina and pupil, and yet that it was not one of general paralysis. It cannot be said that it was one of those rare instances of apparent recovery (which are, in fact, merely remissions), for we have the history of intervals of seven and three years between the recorded attacks.

Within the last two years I have met with another almost identical case, save that the delusions were not so extravagant, and that the motor impairments were more pronounced. In this instance also the ophthalmic indications of general paralysis were not presented. Guided by their absence and a clear history of heavy drinking, my opinion was guarded, and I was saved the opprobrium attachable to a false diagnosis. The patient recovered mental and physical health within six months, and is now well.

In many forms of insanity, particularly in general paralysis, it is often extremely difficult to determine whether bouts of drinking are the cause or the prodromal symptoms of the disease; but, in the two cases now under consideration, this difficulty does not present itself, for in both instances the attack appeared suddenly and synchronously with the drinking. We not unfrequently meet with cases of acute but transient alcoholic insanity, presenting many of the features of general paralysis, although not to such a marked extent as in the two just spoken of; in such we have the tremulous tongue, the uncertain gait, and a certain degree of optimism and exaltation, which pass off on the withdrawal of stimulants. Correlating such cases with those I have just described, I cannot resist the conclusion that acute alcoholic insanity occasionally, although rarely, strongly simulates the graver disease.

Blandford asserts that the paralysis of chronic alcoholism occasionally resembles general paralysis of the insane in the loss of memory, of muscular power, and of co-ordination, but differs in the mental symptoms and in the non-impairment of the function of articulation: he says, "Except for the absence of the stutter, the muscular defect may be the same, but the mental symptoms are rarely alike, and if you make a close investigation of the commencement of the disorder, its history, and supposed cause, you can rarely make a mistake" (*op. cit.*, p. 279). When we consider that the history and supposed causes of the two forms of insanity are very frequently the same,—that is to say, in many cases of general paralysis we can trace alcoholism as the efficient cause,—it cannot be said that these guides to diagnosis are very serviceable; and when it is added that, at least in Scotland, the stuttering speech and mental exaltation are found in chronic alcoholic insanity, it is evident we must seek for some more pathognomonic indication on which to found opinion.

It is needless to occupy the time of the Society by narrating in detail the history of four cases of chronic alcoholism, simulating general paralysis, which have within the last five years come under my observation; suffice it to say, that, in all, the character of the symptoms was such as to preclude the enunciation of a definite diagnosis,—defective articulation, optimism, and impaired co-ordination were as well marked as in the most characteristic case of general paralysis. Three of these cases are still alive in exactly the same condition as when they first came under observation; the fourth died of Bright's disease three years after the incidence of insanity without developing any further symptoms than those mentioned above. In a fifth, which was transferred to the Fife and Kinross Asylum eleven years ago, certified to be one of general paralysis by a specialist for whose opinion we all have great respect, nothing but the lapse of time enabled me to come to a conclusion as to its nature; there was nothing militating against the diagnosis of general paralysis, except that the man gradually improved, was discharged, and for years after supported himself by his handicraft.

In the very large majority of cases uncomplicated with alcoholism, in which general paralysis is suspected, there exists no difficulty in arriving at a definite diagnosis, but in those in which this additional factor comes in, and more especially when its presence is denied, the solution of the problem is by no means so easy. We may be very considerably assisted by the presence of sensory paralysis, which, as Russell Reynolds shows, is liable to occur in advanced cases of alcoholism; but even this indication is not perfectly trustworthy, for in general paralysis I have met with cases in which reflex action was much diminished, although, as a rule, in the early stages it is not affected. The indication which weighs most heavily with me is the condition of the pupil and retina. If, after careful weighing of the history and symptoms of a suspected incipient case, we find contracted or irregular pupils, and, on ophthalmoscopic examination, hyperæmia of the retina, the weight of evidence leans towards general paralysis; if the pupils are unaffected, and the fundus of the eye is anæmic, even although there may be some slight degree of atrophy of the discs, chronic alcoholism may be suspected, and diagnosis made with greater caution. I do not say this is an absolute diagnostic, for Clifford Allbut observed no ophthalmic change in five out of fifty-five cases of general paralysis; it is open to question, however, whether these five were not the subjects of chronic alcoholism simulating the other disease.

It is true that, occasionally, in chronic alcoholism, hyperæmia of the retina is observed, but this is, curiously enough, only another guide to diagnosis, for on the withdrawal of stimulants a marked improvement takes place in a month or six weeks, and recovery in six months; whereas in the general paralytic the retinal vessels continue hyperæmic when the other symptoms of the disease have materially abated in intensity.

As an aid to diagnosis the colour-test of Leber may be employed. This authority states that there exists in all cases of *amblyopia sine materia*, *i.e.*, amblyopia dependent on alcohol, tobacco, and other poisons, a colour scotoma in the field of vision, the centre of which is between the point of fixation and Mariotte's blind spot. The scotoma is of horizontal form, its long diameter being horizontal. In the centre of the scotoma a red disc usually appears of a blackish or grayish colour, and towards its circumference or at its centre, where the degree of blindness is slight, the disc appears yellow. In such cases a green disc appears yellow; but blue and yellow are distinguishable. The method of applying the test is to cause the patient to gaze intently on a black spot marked on a sheet of paper, held a foot from the eye, and to move a coloured disc to distances varying from half an inch to three inches from the spot, instructing the patient to state the variations in colour produced by the change of position. In two cases which I have examined with Dr Argyll Robertson, to whom I am indebted for the suggestion, the results have been in accordance with the statements of Leber. I have likewise applied the test to two cases of general paralysis, uncomplicated with alcoholism, in which the state of mind does not preclude trustworthy observation, both being in the improved condition which so often supervenes on the abatement of primary excitement. In both, the retina is in a state of hyperæmia and the pupils contracted ; the red disc appears very dark near the black spot, lighter as it is removed away from it.

It may be fairly stated that there is no one pathognomonic symptom ; the diagnosis between alcoholism and general paralysis must depend on three conditions, which, when occurring, or rather concurring, in a case may determine our opinion. When we have impaired reflex action, regular and normally-sized pupils, and transient hyperæmia of the retina, we may diagnose alcoholism : when converse conditions exist, general paralysis of the insane. The colour-test may serve to confirm diagnosis.

I trust enough has been said to show that the diagnosis of general paralysis is not such a patent matter as it is generally represented to be. I have confined myself in this short paper to the question of the occasional great similarity between it and alcoholism, acute and chronic; I purpose on another occasion to lay before the Society the difficulties presented by the resemblance of symptoms of other forms of nervous disease, especially those complicated with syphilis.

## ARTICLE II.—*On the Condition of the Cervix Uteri in the Latter Months of Utero-Gestation: with a Specimen.* By ANGUS MACDONALD, M.D., etc.

(*Read before the Obstetrical Society of Edinburgh, 28th February* 1877.)

WRITERS on diseases of the skin are occasionally wont to state that diseased conditions of the cutaneous system being exposed to the critical eye of the physician ought, of all affections, to be best and most widely known, and yet it is rather the rule to find a lamentable want of knowledge regarding them in the general medical mind. In imitation of these authors, we might say that the cervix being that part of the uterus most readily approachable both for diagnostic and therapeutic purposes, obstetricians ought to be specially well acquainted with its anatomy, physiology, pathology, and therapeutics. But is it not the fact that fully as much uncertainty pervades the profession regarding the cervix as regarding the body of the uterus ? How painfully frequent are totally contradictory views inculcated by authorities of equal eminence regarding its diseases and their treatment, and how often are its functions confounded with those of the lower uterine segment, and *vice versa !*

However, in the present paper I do not mean to go into considerations of this kind, but merely to restrict myself to a single point, viz., the behaviour of the cervix during the latter months of utero-gestation.

I need hardly say that, up to the time of Weitbrecht, 1750, it was universally held that the cervix uteri during pregnancy underwent a gradual amount of shortening, which was believed to be so closely correlated to the period of the pregnancy that the shortening of the organ might be taken as a symptom of the period of the pregnancy. It was believed, indeed, that during the early part of pregnancy the body alone was concerned in forming a receptacle for the fœtus, but that as pregnancy advanced the cervix at its upper part became gradually used up, and was made to contribute successive portions to the amplification of the lower segment of the gravid uterus, until about the natural time for the onset of labour nothing remained of the original cervical cavity except a ring forming the outer os.

The description given by Weitbrecht of a uterus in the eighth month of pregnancy, however, ought to have been sufficient to settle the matter. But, like much other good work, his observations were soon forgotten. Stolz and Caseaux, from observations upon living women near the time of labour, again directed attention to this subject, after the oblivion into which it had unworthily fallen. But it is Duncan[1] to whom the merit of really resuscitating the true views in modern times is due. In his masterly paper on this subject he gathers together the earlier views scattered through works too seldom consulted now-a-days, and furnishes drawings of several dissections from earlier authors, such as Roederer, to which he adds several from dissections made by himself. These drawings from Roederer and John Hunter are especially valuable, as they were correctly made by the authors according to what they found in nature, and are dead against their own defective theoretical ideas.

The views promulgated by Duncan in this country and supported by Müller in Germany may be said to have almost attained to absolute acceptance on the Continent and to very general currency here. The advantage in the position taken by him is that it rests not on theory, but on anatomical fact. One fact is surely worth more than a very large amount of theory. And yet it is astonishing how difficult it is for the honestest and best observers to get rid of the slavery of preconceived conceptions, and away from the feeling that their plans are so much better than what is said to be found in nature, that the latter must be interpreted and explained away so as to bring it into agreement with their fancies.

It is accordingly rather disappointing to find an author of such

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[1] Researches in Obstetrics, p. 243.

deservedly great reputation as Bandl striving to return to the antiquated notion that the cervix uteri is used up in the latter months of pregnancy to aid in forming the lower uterine segment. Bandl's notion is difficult to appreciate, but seems to me to be an attempt to reconcile the old mistakes with modern facts. I have not been able as yet to get the paper he published last year on the subject, but the following is a translation of that part of his communication to the gynæcological section of the Association of German Natural Philosophers, held at Hamburg last year, so far as it bears upon the subject under discussion at present. Bandl is describing the dissection of the uterus of a woman who died in the 8th lunar month, upon whose body Cæsarean section had been performed. "The mucous membrane canal (of cervix) remains 2–4 centimetres long, with its surrounding tissues intact, as Müller has described it, up to the initiative pains, often also even many hours during the labour. Outside and above this canal there is formed, indeed, towards the end of pregnancy, out of the lower part of the uterus and the muscular walls of the cervix, and also partially out of the upper portions of the vagina, under continuous growth and exalted activity of the parts, *an entirely new cervix*, and the small still existing canal, with its internal os, plays in the matter a quite subordinate part. It is this new cervix which the table of Braune shows, viz., the variously described but still undefined lower uterine segment of the older obstetricians. This new cervix has a line of separation from the body of the uterus, which is distinctly to be felt, lying chiefly at the inlet of the pelvis. This line of separation corresponds to the second uterine os, as described by many authors, and the latter is only identical with the os internum of Braune's table, identical with the os internum which one feels a handbreadth above the os externum, when one passes it into the cavity of the uterus to perform version or to separate the placenta."

It is satisfactory to find that these erratic views did not find a single supporter at the meeting, but, on the other hand, met with unqualified opposition from every gynæcologist present.

It is worthy of notice that Bandl finds the original cervix persistent in his cases, but, having taken up the theoretical notion that the tissue of the cervix as we find it in the latter period of utero-gestation is too small in quantity to be dilated into the cervical zone of the genital passage, as we find it, after the cervix is completely opened up, he feels himself constrained to hypothize the airy structure, an account of which I have translated for you.

But that views of an extremely indefinite character are held by justly distinguished members of our profession at home on this subject, is evidenced by the note on this point added by M'Clintock[1] to his edition of Smellie, just published. According to this note, one is inclined to believe that he holds the condition of the cervix during the latter months of pregnancy to be still undetermined, and thinks that the most valuable contribution to its solution is a dissection described by Dr Arthur Farre, in the "Cyclopædia of Anatomy and Physiology," where there would appear to have existed, so far as I can gather, a cervix abnormally relaxed, if not already partially developed, in a phthisical subject. It is not stated, however, whether there had been pains set agoing by the dyspnœa, which, we may suppose, preceded death in such a case, though we know that under such circumstances labour pains are apt to be set up. Before weight could be placed in such a recorded case, every particular would need to be known respecting the death. In this case the cervix seems to have been opened up at the inner os and throughout its cavity, so as to form an oblong spheroid, and to permit the head to rest upon the outer os.

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[1] Smellie's Midwifery. Edited by M'Clintock, vol. i. p. 187.

Now it so happens that, about a year ago, one of my dispensary pupils was called to attend a patient in High Street, who believed herself actually in labour at full term. He saw her in the evening he was sent for, and found no pains present, and that she was mistaken in believing her labour had begun. On the forenoon of the following day, on sitting up in bed, she suddenly died. I insisted on a sectio, and with some difficulty procured the consent of her husband to the removal of the entire uterus with its contained ovum. The post-mortem examination was performed for me by Dr Wyllie, and the uterus was injected by Professor Turner from both the uterine and fœtal vessels. One-half of this uterus is now laid before you. It is to be noted that the patient was forty years old, and that she had given birth thirteen times previously; that she supposed herself arrived at the full term of pregnancy, and, indeed, in labour, so that she must at least have been past the middle of the ninth month, and that the uterine organs were in a condition of absolute health. There was found no apparent cause of death on the sectio, except it had been sudden syncope from a somewhat large, pale, and friable heart. In this case, therefore, no question of the effects of disease can be raised.

At the examination it was noted by Dr Duncan, who was also present, that the cervix was undeveloped, and the inner os allowed two fingers to pass. I leave the members of the Society to judge whether there is any evidence here either of the obliteration of the original cervix, or of the formation of a new cervix, such as Bandl would make us believe, or of a squashing down of the cervix by an opening out of the upper part of it, as Farre and M'Clintock would seem to believe in. Bandl's line of demarcation between his new cervix and the body of the uterus is conspicuous only by its absence. It will be noted, that the mucous membrane of the cervical cavity is still exactly as it had been throughout the pregnancy. The *arbor vitæ* arrangement is still seen, and the line of demarcation between the decidua and the cervical mucous membrane is as perfect as it could have been at any time during the pregnancy. The membranes have been raised by Mr Turner from the mucous membrane for a little space upwards from the line of junction between the cervical mucous membrane and that of the body merely to show the decidua, but they were originally found intact and attached down to the very edge of the inner os. The length of the cavity of the cervix is, at its longest part, where the lips project most, $1\frac{1}{2}$ inches; at the junction of the one lip with the other, it measures only $1\frac{1}{4}$ inches. The thickness of the cervical tissues amounts to $\frac{7}{16}$ths of an inch, which is decidedly greater than that of the body of the uterus, which measures only a quarter of an inch in thickness. Of course the dimensions are no doubt lessened in all directions by the preparation being kept in spirit for a considerable time.

I have further to observe, that the vaginal portion of the cervix is still markedly persistent, and that the thickness of the cervical wall, with the unused cervical portion, lend a considerable probability to the conviction, if that were needed, that Bandl's fears about want of tissue out of which to form the cervical segment of the fully dilated genital passage are groundless.

I have thus laid before the Society this very interesting and rare specimen, and do not mean to enlarge further on the subject than to state, that it seems to me to put beyond a doubt that, under ordinary circumstances, the cervix uteri neither shortens nor has its cavity obliterated during pregnancy. On the other hand, it manifestly shows that the cervix, while partaking of the general hypertrophy of the genital organs during pregnancy, maintains its general shape intact till very near the termination of pregnancy. It also lends no support to, or rather flatly contradicts, the idea of Bandl, that there is a new cervix developed above the real cervix during the latter months of pregnancy. I confess myself quite at a loss to comprehend the notion of using up the cervical tissue to amplify the lower segment, whilst the mucous membrane of its cavity remains intact, as put forward by Bandl and others. My difficulty is more especially great, as in my case the tissues of the cervix, instead of being less large, are seen to be fully as large as one could have expected to find them. This view seems very much on a par with the now exploded chemical theory, that heat was the principle of lightness; for here we find the cervix lose tissue and yet get larger. But, as in the chemical case, when the process of oxidation became better understood, the lightness theory collapsed; so I rather fear that under the progress of anatomical observations, Bandl's and all similar theories must likewise ultimately disappear. Nor does this specimen support the views of Farre and M'Clintock, that there is an opening up of the cavity of the cervix, and a flattening out of the organ in the latter months of pregnancy.

There is a further question that is not touched so directly by the condition of the specimen before us, although it is raised by the consideration of it, viz., At what period during pregnancy does the cervix uteri begin to be developed or opened up? It is usually stated that this takes place within the last fortnight or ten days of pregnancy. But the observations regarding the process have hitherto been rather defective.

It is, of course, difficult to obtain satisfactory observations. Those of Stolz and Caseaux are undoubtedly among the best. Caseaux, "Traité de l'art des accouchements, 1853," page 106, says, in speaking of the common belief of the cervix being gradually used up to amplify the lower uterine segment in the latter months of pregnancy, " There is here, I fear not to say, a complete mistake, pointed out, however, since 1826 by M. Stolz, and to which I have myself never ceased to call attention since 1839. No, the neck is not shortened, as has been so long maintained, it preserves its entire length up to the last fortnight of pregnancy, and it is easy, above all in women who have already borne children, to prove this, as we shall show further on. But in the last weeks its length, hitherto intact, diminishes very rapidly, ends even by complete obliteration, and we shall tell soon what is the very simple mechanism of the disappearance of the cervix some days before labour."

What Caseaux refers to is the silent contractions which take place usually for a week or ten days before the onset of labour-pains (see his footnote, page 413, *ibid.*). But, though the statements of Stolz and Caseaux are, on the whole, correct, the evidence on which their opinions rest is very unsatisfactory, as it is derived from examinations of the soft and yielding cervix near or at the onset of labour, when the tissues are so soft and yielding that it is difficult to measure them accurately. Indeed it would appear that Stolz and Caseaux were both led thus into the error of predicating too frequently a barrel or spindle shaped cervical cavity in the latter months, a condition that no doubt at times exists, but the frequency of which they have, I think, greatly exaggerated. It is easy to make such a shape of the cervical cavity appear to exist owing to the mere act of examination approximating the outer to the inner os.

It is accordingly with considerable satisfaction that I have read the recent contributions of such an able and accurate observer as Litzmann[1] on this point; and I cannot help summarizing and commenting on his researches on the matter as a conclusion to this paper. We have long known that the cervix was prepared for dilatation by natural softening and succulency, due partly to its sharing in the general hyperæmia of the genital organs near the termination of pregnancy, and partly also to the venous stasis occasioned by the pressure of the presenting part upon the lower uterine segment against the pelvic inlet. But on this condition of preparation we have superadded active stretching of the canal of the cervix near the termination of pregnancy, due chiefly to what are called silent contractions, these being usually painless rhythmical contractions of the uterus occurring within an indefinite period before the onset of what is usually called labour, *i.e.*, when the pains become so severe as to attract the patient's attention to them.

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[1] Archiv für Gnäkologie, Bd x. s. 118.

When this process of opening up of the cervix begins, is, however, as a matter of exactly proved science, as yet far from ascertained, notwithstanding strong assertions similar to these I have recorded from Stolz and Caseaux, and notwithstanding also that it is a general conviction in the professional mind.

Litzmann observes that the cervix uteri at the inner os tends to be dilated from two causes during the latter months of pregnancy: 1st, The ovum continuing to grow after the uterus has ceased to develop new muscular fibres, *i.e.*, after the seventh month of pregnancy, produces consequently stretching of the whole uterine cavity, and greater tension in its walls; 2d, More especially is this the effect of the contractions that occur most often without pain during the same months. These changes have for their effects the production of a horizontal strain upon the inner os, by which it is made to open up, and also the displacement downwards of the apex of the ovoid formed by the ovum, so as to tend in the same direction, and to push open the os. If the latter effect now occurs in any considerable degree, as there must be displacement downwards of the apex of the ovum, and at the same time displacement upwards of the part of the lower segment of the uterus adjoining the inner os, there must arise straining of the membranes along the line of attachment, and separation of them from the surface of the body, for a variable portion around the inner os.

If now the cavity of the cervix is traversable to the examining finger, we ought to have, in the attachment or separation of the membranes in the neighbourhood of the inner os, a means of testing our observations as to whether the inner os is partially opened up, or still intact. If we find the membranes running down in their attachment to the line which, to the examining finger is indicated as the upper limit of the cervix, then we should be certain that the inner os is still intact, and we are in a position to measure the length of its cavity with more or less accuracy. Again, if the membranes are found separated to more or less extent around what seems to be the upper limit of the entire cervix, we are certain that the cervix is more or less already developed, and that a portion of its cavity is wanting.

The outer os, again, may and does dilate independently of these active forces, and in consequence mainly of the strain upon its edges produced by the weight of the uterus acting through its vaginal and pelvic attachments.

The opening up and dilatation of the cervical canal thus results in the latter months of pregnancy in a twofold direction from the outer os as well as from the inner os. It is also observed by Litzmann that the tension of the walls of the uterus attains in primiparæ in the two last months of pregnancy a higher degree, on an average, than in multiparæ, and that contractions in the former are more frequent, and probably begin earlier than in the latter.

The outer os remains shut, or is only slightly opened—is, at all events, not traversable to the finger, specially in primiparæ, in contradistinction to multiparæ, not only more frequently, but also up to a later period of the pregnancy. In Litzmann's protocols, this condition is noted in primiparæ up to the 36th week of pregnancy in the preponderating majority, from the 36th to the 39th week in an average of two-thirds, in the 38th week even in two-fifths, and in the 40th week in nearly one-third of all the cases. In multiparæ, on the other hand, it never was observed after the 38th week—even in the 37th and 38th week only seldom, scarcely in the eighth part of the cases; before this period somewhat more frequently, perhaps in one-fourth of the few observations made.

The opening up and unfolding of the cervical canal upwards from the external os occurs in multiparæ more frequently and at an earlier period than in primiparæ.

In the latter it was never observed before the 34th week of pregnancy—very seldom, and only exceptionally, in the 34th and 35th weeks; from the 36th to 39th in fully one-fifth of the cases; after this period, in proportion as the complete traversability became more frequent, again in diminishing frequency.

In multiparæ, on the other hand, the outer os was found from the 33d week invariably very much more frequently opened than shut, from the 36th to the 39th week this relation, in comparison with the complete traversability of the canal, formed still the rule, until from this time onwards the latter becoming more and more frequent obtained the preponderance.

Complete traversability of the cervical canal occurs in general likewise more frequently in multiparæ than in primiparæ—in both, however, not before the 36th week of the pregnancy.

But whilst in primiparæ it was observed from the 36th to the 39th week only quite rarely and in occasional cases, in the 39th week in perhaps two-fifths, and in the 40th week in not quite two-thirds of the cases, it was found in multiparæ from the 36th week up to the 39th in somewhat more than one-third of the cases, in the 39th week in probably three-fourths of the cases, and in the 40th week almost without exception. This condition presupposes always a considerable softening of the cervix throughout its entire length.

But the part to which I specially want to draw the attention of the Society at present, as it bears most directly on the subject in hand, and is merely led up to by these observations I have translated and summarized, are the following analyses of 81 observations on women whose cervices were completely traversable to the examining finger.

These included 35 observations in primiparæ and 46 in multiparæ, which are kept separate in the analysis.

Of the 35 observations in primiparæ, Litzmann found 9 in which the uniform narrowness of the cervical cavity, or its contraction at its upper part, prevented the examining finger from detecting whether around the internal os the ovum was separated or not.

Of these observations, 1 was made in the 36th week,
1 „ „ 38th „
6 „ „ 39th „
and 1 „ „ 40th „

The length of the cervical canal varied from $\frac{6}{8}$ths to 1$\frac{5}{8}$ths inch.

In 5 other cases the apex of the ovum was scarcely observably separated, or, at most, for a very small distance along the circumference of the internal os. These observations were made—

2 in the 39th week,
and 3 „ 40th „

In one of them the cervical canal measured about 2 inches. In the rest, however, its extent did not exceed $\frac{6}{8}$ths to 1$\frac{1}{8}$th inch.

In 21 cases the membranes were found already separated to a pretty considerable extent. These observations were made—

1 in the 36th week,
1 „ 38th „
2 „ 39th „
and 17 „ 40th „

and all before the onset of distinct labour-pains.

The cervical canal was, as a rule, markedly shortened. Only in some few cases did it have still a length of 1$\frac{1}{8}$th to 1$\frac{5}{8}$ths inch. It mostly measured not over $\frac{1}{5}$th to $\frac{2}{5}$ths inch. The less of the cervix that was developed, the more marked was the line of division to be felt between the undeveloped and the developed portions of its cavity. In some instances the cervical wall at that line projected inwards at an acute angle. Conversely, the more completely the cervical cavity was opened up, the less pronounced was the line of demarcation found.

Among the 46 observations on multiparæ, in 5 only was the narrowness of the canal a hindrance to the ascertainment of whether the apex of the ovum was separated from, or adherent to, the uterine surface adjoining the inner os. These were made—

1 in the 37th week,
1 „ 38th „
2 „ 39th „
and 1 „ 40th „

In 23 cases the apex of the ovum was either not at all, or only in the immediate neighbourhood of the inner os, separated from the uterus. These cases were examined—

1 in the 36th week of the pregnancy,
2 „ 37th „ „
5 „ 38th „ „
4 „ 39th „ „
and 11 „ 40th „ „

Only in one case did the cervical canal appear shorter than usual. In all the rest it was found over $\frac{6}{8}$ths to 1$\frac{1}{2}$ inch, and even up to 2 inches.

Lastly, in 18 cases the membranes were found separated to a considerable extent from the uterus. These observations were made—

1 in the 37th week of pregnancy,
1 „ 38th „ „
4 „ 39th „ „
12 „ 40th „ „

The cervical canal in these cases was often $\frac{6}{8}$ths to $1\frac{3}{16}$ths inch in length, but it was frequently much shortened.

It is difficult to over-estimate the value of such observations in settling the question with which we started. Much credit is due to Litzmann for suggesting the importance of the degree of closeness of the attachment of the membranes around the inner os as a means to establish the commencement or not of development of the cervix at its upper extremity.

Of course individual differences, both as to length of the cervix and its consistence, still introduce numerous sources of error. But, nevertheless, those who are fortunate enough to hold appointments in a Maternity Hospital ought to get over them by care and time, so as to be able to arrive at tolerably correct averages in regard to the period of onset of the opening up of the cervical cavity from within.

It is all the more important to be able to attain some degree of accuracy in the solution of this problem in the living woman, as it must be a slow process to collect the necessary data from dissections, such as the one now laid before you.

It appears to me sufficiently proved from Litzmann's observations that the cervix uteri at its upper pole is earlier interfered with in primiparæ than in multiparæ, and that this consequence is the resultant of two factors: being due, 1st, to the greater tension of the uterine walls; 2d, to the earlier onset and more frequent occurrence of the silent contractions. It is found by Litzmann that the apparent feeling of the upper limit of the cervix is very untrustworthy, as it may correspond either to the upper limit of the true cervix, or of only its undeveloped portion. Likewise that although in a small proportion of cases it would appear that development of the cervix at its upper pole begins as early as the 36th week in primiparæ, it seems at least a week later before this ever occurs in multiparæ; and that it is very much more common in both classes of cases to find the cervix practically intact even in the 40th week.

But let us proceed to scan these figures a little more closely. We are, I think, fairly entitled to hold that those cases, in which Litzmann failed from narrowness of the cervix to ascertain whether there was separation of the membranes around the inner os or not, were in all probability cases in which the cervix was still intact. And those amounted to 14 out of the 81 cases, 9 being primiparous, and 5 multiparous, 1 being observed in the 36th week, 1 in the 37th, 2 in the 38th, 8 in the 39th, and 2 in the 40th. Again, in those cases in which, by direct observation, the membranes were proved to be not at all, or only to an extremely small amount, separated around the margin of the inner os, we need no argument to support the practical persistence of the cervix. And these observations included 28 cases out of the 81, 5 being primiparæ, and 22 multiparæ. In regard to the period of observation, 1 was made in the 36th week, 2 in the 37th, 5 in the 38th, 6 in the 39th, and 14 in the 40th week of the pregnancy.

If now we put these two sets of cases together, we find that in 42 out of 81 observations made on patients, the cavities of whose cervices were traversable completely to the examining finger, the inner os was scarcely, if at all, developed during the latter month of utero-gestation, 2 observations being made in the 36th week, 3 in the 37th week, 7 in the 38th week, 14 in the 39th, and the surprising number of 16 in the 40th week of the pregnancy, 14 cases being primiparous, and 28 multiparous. This result shows that in these cases the tendency to persistency of the cervix up to the extreme limit of utero-gestation was distinctly greater in multiparæ than in primiparæ, for of 46 multiparous cases, 28 were not developed at this late period; whilst of 35 primiparous, 14 were not developed, so that the tendency to late development of the cervix at its upper pole is greater in multiparæ than in primiparæ in proportion as $\frac{28}{46}$ is greater than $\frac{14}{35}$, or nearly as two-thirds are greater than two-fifths.

But though this is only in accordance with observations such as I have now the privilege to lay before you, it is quite at variance with such assumptions as Farre and M'Clintock bring forward. For it is exactly in multiparous cases in which there occur that general softness and succulency of the parts, which those authorities wish us to believe is caused by opening up of the inner os during the latter months; and yet those accurate observations prove that in these very cases the cervix is specially slow to develop from the inner pole.

Coming now to the remaining 39 observations, 21 of which embrace primiparæ, and 18 multiparæ, in which the cervix was found at its inner pole more or less completely developed, we notice that 1 observation was made in the 36th week, 1 in the 37th week, 2 in the 38th week, 6 in the 39th week, and the very large number of 29 in the 40th week of the pregnancy. These observations, it is to be noted, were all made before the onset of manifest labour-pains. It will be noticed, that of those cases in which the cervix was developed to a considerable amount at the time of examination, there are 21 out of the 35 primiparæ, and only 18 out of the 46 multiparæ, or three-fifths of the one set, and only two-fifths of the other.

We thus notice that in 42 out of 81 observations the inner os was found persistent, or all but so in 42, whilst it was more or less opened up in 39; and yet all these observations were made within the last month of pregnancy, 14 and 16 of the former class being made in the 39th and 40th weeks respectively, and 6 and 29 of the latter.

These exact observations therefore bear out in the most complete manner the general statements of those authors who hold that the cervix uteri as a rule retains, so far as its canal is concerned, its complete integrity till ten days or a fortnight before labour. Indeed they seem to go further, and to indicate that in a large proportion of cases the inner os is still undeveloped and the cavity of the cervix not shortened in the 40th week, that is immediately before the onset of labour-pains. They also show that, if we divide those cases in which the cervix is developed more or less completely before the commencement of labour into primiparæ and multiparæ, we should find that about 60 per cent. of the primiparæ had the cervix more or less shortened from above downwards, but only about 40 per cent. of the multiparæ.

It may be objected, perhaps, that I assume too much in claiming that those cases in which the condition of the cervix prevented an accurate opinion being obtained of the presence or absence of separation of the membrane from around the inner os at the time of examination, ought to be classed as cases of persistency of the inner os at the time. Still, though they were divided between the two sets of cases, their relative paucity would not much alter the result.

I am prepared, however, to admit that the number of actual observations are too few to permit of our forming any sweeping inductions. Still they seem to me extremely valuable as leading the way towards obtaining correct data on which to proceed with the solution of this interesting problem.

And, furthermore, it is to me extremely interesting to be able to say, that those observations of Litzmann would have warranted us in expecting with great probability that, in this poor woman's case, we should find what anatomical evidence has demonstrated to us, viz., that though she was believed to be at full term, yet the inner os was more likely than not to be in a still undilated condition. When thus deductions from clinical observations are found to be established by anatomical facts in matters of this kind, we find the strongest evidence of the correctness of the clinical method by which the data for the deductions were obtained.

As the result, therefore, of the above observations, and from a consideration of the specimen before us, and of others that have been brought forward, we are entitled to affirm—

That the cavity of the cervix uteri does not shorten during pregnancy, except within the last month, but rather lengthens; and that observations founded on its supposed shortening are entirely erroneous. I do not mean to deny that there is an apparent shortening of the cervix uteri, as ascertained by the examining finger as pregnancy advances. But what I mean to affirm is, that this so-called clinical fact, when tested by accurate observations, whether on the dead body or the living subject, is a complete delusion. The mistake is the resultant of several concurrent factors, being partly due to varying heights of the pregnant uterus, partly to changes in the bulk and curvatures of the cervix, and particularly to the increasing softness of its tissues, which diminishes the sense of resistance, and thus gives the idea of shortening to the examining finger. Still, it is by no means denied that a certain amount of accuracy in judging of the advance of the pregnancy from the apparent shortening of the cervix in the latter months is attainable. For there is no doubt but such opinion can be thus obtained, and has been obtained, with a considerable amount of certainty. What is argued in this paper, and what every tittle of anatomical evidence that has heretofore been adduced, with the doubtful exception of Farre's dissection, supports to the utmost, is, that the clinical observation is erroneously explained, there being no actual shortening before the last month of utero-gestation at the earliest. The bleeding in placenta prævia is now pretty generally held to be accidental, more especially since the publication of Duncan's extremely able papers upon the subject; whilst the occasional occurrence of absence of bleeding in placenta prævia up to the commencement of labour at full term, is as strong an argument as could be brought forward in favour of the view of persistency in the cervical canal maintained by me. For if the cervix opened up normally within the latter three months, it is clear that all placenta-prævia cases ought to bleed so soon as this process began. But some cases do not do so. The effect of position over the inner os, leaving, as it does, a certain area of the placental surface unsupported, and subject to increased vascular pressure, is more than sufficient to account for the frequency of hæmorrhage in those cases within the latter months without our assuming any opening up of the inner os. As to the argument that my case was an exception to the general rule, I have only to say, that all other dissections that have been performed agree with it.

That the cervix at its upper pole remains longer undeveloped in multiparæ than in primiparæ. That there is no foundation, so far as the anatomical evidence furnished by the specimen before us extends, for the widespread delusion that the cervix uteri is used up during the latter months in the amplification of the lower segment of the uterus; and still less for the modern form of this opinion lately adduced by Bandl, that we have the formation of a new cervix during the latter months, which cervix is intermediate between the true cervix and the body of the uterus, and formed of tissues derived from uterus, true cervix, and vagina.

Within the last month the cervix may shorten from above downwards at any period, but rarely does so till the 39th or 40th week, and in a large proportion of cases—from Litzmann's observations probably half of all the cases—not then even, but remains persistent till the labour-pains come on; at any rate, during the 39th and even up to the 40th week.

## ARTICLE III.—*On Filiform Bougies: their Uses and Advantages.*

By J. C. OGILVIE WILL, M.D., Surgeon to the Aberdeen Royal Infirmary, and Examiner in Surgery in the University of Aberdeen.

WHEN speaking of the superiority of French flexible catheters to those known as English, Sir Henry Thompson remarks, "It is one of the many extraordinary instances of English conservatism that these instruments are found in so few hands."—(*Diseases of the Urinary Organs.*) This remark may, I think, be equally well applied to the comparatively slight use made of filiform bougies in this part of Great Britain at any rate.

Liston's dictum, that one should know the anatomy of the urethra thoroughly, and pass the instrument accordingly, is still paramount, and the preference for stiff to flexible bougies and catheters is therefore most manifest. Knowing this, and feeling how much needless trouble and anxiety those who hold this doctrine endure from a strict adherence to this faith, and how much uncalled-for suffering their patients must be called on to undergo, I desire to direct attention to the advantages accruing from the employment of filiform bougies in difficult cases of urethral stricture. The cases I purpose relating possess no features of interest for those who have already adopted the use of filiform instruments, but they seem to me to be striking examples of what may be readily accomplished by any one who puts them to the test of practice.

So little notice is taken of filiform instruments by authors of standard works on surgery, that—notwithstanding the powerful advocacy of leaders in surgery both in this country and abroad in works specially devoted to the consideration of surgical diseases of the genito-urinary organs, and in papers in the medical periodicals —it is not to be altogether wondered at that they have not yet found favour with the great body of medical practitioners. Most British authors of works on surgery either altogether ignore the existence of filiform instruments, or, if they do notice them at all, they are dismissed in a sentence or two. Fergusson, Pirrie, Bryant, Holmes, Miller, Druitt, and Fairlie Clarke do not mention them, nor do they find a place in Holmes's System of Surgery; Gant mentions the existence of catgut bougies, while Erichsen speaks favourably of them; Spence alludes to the use of whalebone bougies in cases complicated by false passages; and the author of the article "Bougie" in Cooper's Surgical Dictionary devotes a short paragraph to Plenck's introduction of catgut instruments, but his statement, that catgut has been objected to, as it sometimes expands beyond the stricture, and gives great pain on being withdrawn, is not calculated to encourage a trial of filiform bougies by those who have no practical acquaintance with their use.

Though filiform instruments have received but scant notice in works on general surgery published in this country, still, attention has been frequently drawn to them in various medical journals, and they find a place in Sir H. Thompson's work on Stricture, and he speaks well of very tiny flexible catheters, much smaller than No. 1 (English scale), containing slender steel threads in place of the usual stylet, in the last edition of his "Lectures on Diseases of the Urinary Organs." In France and America filiform instruments are in constant use, and receive their due meed of praise from surgical authors.

Twenty-five years ago Leroy D'Etiolles published a work on the advantages of using twisted filiform bougies, and his example has been largely followed by his surgical brethren in that country. Ch. Phillips, in his "Traité des maladies des voies urinaires," published in 1860, strongly commends the use of filiform instruments, and dwells at considerable length on the advantages attending their employment; and he gives the details of some cases, among them examples of multiple and traumatic strictures, where he succeeded in passing whalebone filiform bougies after Nélaton, who ultimately sought M. Phillips' assistance, had failed; and from a note in the last number of the *Edinburgh Medical Journal* I see that Amussat, fils, has lately written a pamphlet on the subject now under consideration.

The use of filiform instruments is warmly recommended by American surgeons—notably by Bumstead and Van Buren, whose works on Venereal Diseases, and Diseases of the Genito-urinary Organs, are replete with practical information concerning all the minutiæ of treatment proper for affections connected with the genito-urinary tract. In speaking of filiform bougies Bumstead says, "If their value were better known, we should hear of fewer cases of 'impassable stricture;'" while Van Buren remarks, "Filiform bougies, intelligently used, make impassable strictures the greatest rarities in a surgeon's practice." Were it necessary to substantiate the statements of those well-known and widely-experienced surgeons, my own experience would lead me to express an equally strong opinion, and I think that the cases I am about to narrate would prove, in a certain degree at least, that I would be justified in doing so.

Four varieties of filiform bougies are met with, viz., the catgut, the English, the French, and the whalebone. Of the two first I have no personal experience, but those who may desire information concerning catgut bougies will find a valuable paper, describing their mode of employment, by Professor Rawdon Macnamara, in the *Medical Press* for 29th September 1875, and notices of the English instruments—bougies and catheters—may be found in Van Buren's work, and in Sir H. Thompson's Lectures, already alluded to.

The French filiform bougies are made of the same material as the well-known French catheters; some of them have a central wire of lead or copper to allow of their being bent at different angles, while others have a slender piece of whalebone in place of the wire to give them firmness. For my own part, I prefer the simple French instruments without any addition, as, if it is desired to use a bougie bent to an angle, or if a stiffer instrument is required, whalebone will be found to fulfil both indications. Whalebone filiform bougies are thin, bristle-like strips of that material, and may be obtained with or without olive tips; they are extremely smooth and non-irritating. By dipping them in hot water they are rendered pliable, and their points may be twisted into any form desired, which they will retain on becoming cold.

French filiform instruments measure from one millimetre in circumference upwards, while whalebone bougies may be had of a still smaller size. Some are of the same thickness throughout, but most of them are made with the shafts thicker than the points, the tips measuring one millimetre, and gradually increasing to one, two, three or more millimetres as the proximal extremity of the instrument is reached. Flexible catheters which, from their minute size, may be regarded as filiform, are also to be had. I have lately received a supply of these instruments from Krohne and Sesemann, which they tell me are the smallest catheters yet made; they measure rather more than three millimetres at their points, and four at their shafts, and contain strands of very delicate wire in place of the ordinary stylets. They are thus considerably smaller than No. 1 English.

The mode of using filiform instruments hardly requires any notice, as it does not vary in any very important particulars from that followed in the case of other flexible instruments. A little difficulty is sometimes experienced in passing a filiform bougie to the strictured point, as its fine point is apt to become engaged in enlarged follicles so often present in urethræ which have been the seats of inflammations. This difficulty may be easily overcome by first filling the canal with oil, as was done in Case 1, where the points of entanglement were numerous, and not easily avoided.

It may be observed that I used carbolic oil, a practice I now invariably follow when lubricating any urethral instrument, as my friend Mr Chiene has proved to my complete satisfaction, that the danger of introducing germs into the bladder is not to be lightly regarded. When the bougie is fairly within the stricture its further progress may be suddenly arrested by muscular spasm, and it may be so tightly grasped that it may be momentarily impossible either to pass it into the bladder or to withdraw it. When working with so slender and flexible an instrument the use of force would be manifestly out of place, as the result would inevitably be fracture of the bougie or bending it in such a way as would utterly defeat the object in view; therefore the best rule to follow in such circumstances is to leave the bougie in position for a few minutes until the spasm ceases, when little difficulty will be found in guiding it into the viscus.

In some cases, however, after the stricture is traversed by the bougie, it may still be arrested before the bladder is reached, from its point becoming entangled in the meshes of the reticulated mucous membrane lying behind the contraction; if this happens, the bougie should be slightly withdrawn, and a fresh attempt to lodge it should then be made, the instrument being advanced in a rotatory manner, the twisting movement rendering the probability of entanglement much less likely than if it be simply pushed straight onwards. Filiform bougies with angular points are chiefly useful in assisting one in finding the orifice of a stricture when it is placed to one side instead of being in the mesial line; when the strictured point is reached the bougie should be gently twisted from side to side until the point engages itself in the opening, when it may be pushed through the strictured portion of the canal into the bladder. Whalebone bougies with twisted or corkscrew points are useful in traversing long and tortuous strictures. The slender catheters just mentioned are warmly recommended by Sir H. Thompson, on account of their ability to transmit urine, and thus to assure the operator that the instrument has really reached the bladder.

When penetration of a tight stricture has been effected, and the filiform instrument lodged in the bladder, it may be utilized in various ways, on which I shall not enter at any length, but content myself with enumerating them.

1. Filiform instruments may be obtained with little female screw-caps fitted on to their proximal extremities, which can be attached to flexible catheters fitted with male screws at their points. Mr Teevan's instrument, so constructed, is a valuable one, and may be used with good effect when stricture is complicated with urgent retention of urine; the catheter, after attachment, being guided into the bladder by the bougie, which curls up in the viscus. After the bladder is completely emptied, the catheter portion of the apparatus should be withdrawn and detached from the bougie, the latter being left in the urethra for the purposes of continuous dilatation, or to allow of the re-attachment of the catheter should it be again required. American surgeons sometimes use fine silver instruments screwed on to filiform bougies in the same manner. Of an instrument, of which he gives a woodcut, and which is very similar to that sold in this country as Teevan's, Phillips says, "Cet appareil, d'une grande simplicité m'a été très utile" (*loc. cit.*, p. 441).

2. Whalebone filiform instruments may be used as guides over which a catheter, either silver or flexible, open at its point, may be passed through the stricture; or a tunnelled catheter, *i.e.*, a catheter with a little bridge of metal on its convexity and close to its point may be threaded over a filiform instrument, and thus be conducted to the bladder.

These latter methods, though occasionally useful, will not probably receive general adoption, as they necessitate the use of much longer filiform instruments than are generally employed. Van Buren and others hold that the guide should be two feet long so as to allow of gradual retraction while the catheter is being passed onwards towards the bladder, lest the whalebone should get doubled up and broken in the canal. These long and somewhat unwieldy strips of whalebone are much more difficult of management than the ordinary-sized filiform instruments.

3. Filiform instruments may be attached, as already described, to instruments used for divulsion, or internal urethrotomy, or to the staff used in Syme's operation.

4. They may be used as guides in external perineal urethrotomy, the mode of operation originally suggested by Van Buren, and perfected by Gouley, being followed, or the modification described by Teevan being adopted (*British Medical Journal*, 1876, vol. ii. p. 422).

5. If the crisis of retention of urine is not very urgent, a filiform bougie, *per se*, may be left in, as it will, in a very short space of time, act as a guide to the urine, which will pass by its sides, and the patient will soon be enabled to empty his bladder in this way without the aid of a catheter. Phillips employs a somewhat similar mode of treatment in cases of retention; he introduces a whalebone filiform instrument, leaves it in for a few minutes, then withdraws it partially, the withdrawal being followed by a squirt of urine, and he goes on repeating this manœuvre until the contents of the bladder are sufficiently evacuated to relieve the agonies of the patient, or until the urine begins to flow by the side of the bougie; and of this he says, " Cette manière d'attaquer la rétention d'urine est longue, peu brillante et peu chirurgicale, selon le sens attribué à ce mot; mais elle est sûre, exempte de danger, et elle donne un résultat assez complet, puisque, en peu de temps, elle fait cesser les douleurs, quoiqu'on n'ait pas évacué la totalité de l'urine" (*loc. cit.*, p. 437). That micturition is readily accomplished during the residence of a filiform instrument within a stricture may be seen from the following cases, and observations to the same effect are set forth in a paper by Mr Furneaux Jordan in the *Lancet* for 29th January 1876. In Case 2, mentioned below, the patient had a better stream soon after the tying-in of the bougie than he had had for many years. Another advantageous mode of employing filiform instruments does not come under any of the foregoing heads, viz., their usefulness in cases of stricture complicated by false passages. In cases of this kind filiform bougies may be used to fill the false passages, first one instrument being passed in and left in position, then another, and so on. After all the openings are filled with bougies, each one should be tried in turn until that one lying in the mouth of the stricture is found, which will be at once known by its being grasped, then

the other instruments are withdrawn, and the remaining bougie pushed into the bladder.

Such, then, are the main uses of filiform bougies, and from what has been pointed out, I think that it must be ceded that they are at least worthy of more notice than they have yet received at the hands of British surgical authors; and it does not seem to me to demand a great amount of comprehension to understand that a minute flexible instrument will worm its way through a stricture with greater ease to the surgeon, *and far less risk to the patient*, than an inflexible metallic probe, be its point never so fine. In the case of a master such as Liston was, surgical miracles may perhaps be accomplished; but this cannot be expected of those to whom the treatment of stricture is somewhat a rarity, and on them I would strongly urge a trial of filiform instruments, feeling assured that they will speedily become converts to the doctrine that flexible instruments are to be infinitely preferred to metallic, and that impassable stricture is more a thing of the past than of the present. Thus they will be enabled to endorse Mr Syme's statement "that if the urine passes out, instruments may always, through care and perseverance, be got beyond the contraction."—(*Edinburgh Monthly Journal*, June 1851.)

The cases which I append only illustrate the simple use of the bougie: they followed one another in rapid succession, treatment being first commenced in two of them within a few days of each other—a circumstance I mention, as it shows that cases demanding the use of very small instruments are not very rare; and they were all cases where even "patience, perseverance, and sweet-oil"—the triumvirate required according to an old surgeon mentioned by Professor Spence—would probably have failed in attaining the desired end, had the instruments not been filiform, or the surgeon much more experienced than myself.

Case I.—G. M., æt. 54, was admitted into Jacob's ward on 7th December 1876, suffering from symptoms of stricture of the urethra. He stated that he had suffered from two attacks of gonorrhœa many years ago, but he was unable to supply any very trustworthy particulars regarding the first appearance of changes in his stream, etc.; it is now distinctly forked, and his calls to make urine are incessant, both during the day and night, and a few drops of water trickle away after the completion of the act. As he is unable to urinate when any one is looking on, his statements could not be verified. On instrumental exploration a stricture was detected in the bulbous region. An attempt to penetrate the strictured portion of the canal was made, first with French flexible olive-tipped instruments, and next with a Thompson's probe-pointed silver catheter, but none of them could be got to enter it. The stricture was evidently an exceedingly irritable one, for the use of the flexible instruments was followed by slight bleeding, and gave rise to considerable pain.

Some days subsequently another attempt was made with various instruments, including Dr Heron Watson's probe-pointed steel catheter, with which I have often succeeded after I have failed with other stiff instruments; but it was again unsuccessful, for I found it impossible to get the point of the instrument within the stricture, the least sensation of grasping never being elicited. Immediately after this trial the case went under the care of my colleague, Dr Garden, as I was prevented by illness from attending to hospital work; and during the next few weeks he made several careful and protracted attempts to reach the bladder, but failed.

On 5th January, after trying the instruments I had before used, and French filiform bougies varying in size from No. 5 to No. 2, and again not succeeding in effecting penetration, though the point of No. 2 was evidently grasped by the stricture, but would not pass through it, I filled the urethra with carbolic oil by means of an ordinary male syringe, and, while retaining the oil within the canal by making pressure at the meatus, I succeeded in passing a whalebone filiform bougie, measuring about half a millimetre in circumference, into the bladder with the greatest ease. (I may here mention that I had before tried to pass a fine instrument while the canal was distended with oil, but in vain.) The bougie was allowed to remain *in situ*, but on account of its extreme fineness it seemed useless to attempt to tie it in. The patient was enjoined to keep quiet, and to prevent the instrument from being washed out during micturition. He did so for some hours, during which he passed water easily; but during an incautious movement in bed the bougie slipped out four or five hours afterwards. Next day a French filiform bougie, measuring No. 3 on the scale-plate, was readily passed and tied in; it was allowed to remain for twenty-four hours, when it was replaced by a No. 1 English catheter. On withdrawing this instrument forty-eight hours after, a succession of French bougies up to No. 16 were passed. Four days after, No. 19 was reached, then No. 21; and at the end of three weeks from the date when penetration was first effected, No. 24 slid in without the slightest opposition. The urinary difficulties disappeared as the stricture expanded.

Case II.—J. B., æt. 36, consulted me concerning a difficulty he experienced regarding urination. His calls to make water were constant, but he never succeeded in passing more than a few drops at a time; and his clothes were continually wet from dribbling. He made water before me, when the usual symptoms of aggravated stricture presented themselves. He had no stream, but sent out his urine in a jerking manner, drop by drop; and at the conclusion of the act, a small quantity ran off when the penis assumed the dependent position. He had been married for nine years, and had had an attack of gonorrhœa about a year before marriage; he was unable to say how long it lasted, but had a vivid recollection of frequent chordee.

Examination with an acorn bougie revealed the presence of a stricture in the bulbo-membranous region. As it was evidently a narrow one, I at once had recourse to comparatively small instruments, both flexible and silver; but, not succeeding with them, I tried filiform bougies, and passed a No. 2 French readily. A French bougie, measuring 1 millimetre at its point, and gradually enlarging to No. 3, was grasped, but refused to enter the bladder. He was told to return in four days, when the instrument before used passed without difficulty, but the next size larger was arrested. I recommended treatment by continuous dilatation for thirty-six hours, which was put in force seven days later. In the interval elapsing between the second visit and the period when continuous dilatation was commenced, widening of the stricture had taken place sufficiently to permit the introduction of a No. 3 French bougie, which was tied in. On visiting him next day he told me, with much delight, that, during the few hours preceding my visit, his stream had been larger than it had been for years before, and pointed with pride to the contents of the *pot de chambre* as an evidence of the success of the treatment. The bougie was removed at the end of thirty hours, and No. 10 French was passed. The after progress of this case was perfectly satisfactory, but need not be detailed here.

Case III.—J. N., æt. 22, admitted into the Aberdeen Royal Infirmary in the end of January 1877, suffering from difficulty in passing urine. Four or five years ago he had two attacks of gonorrhœa, one lasting six, the other two, months. Soon after the second attack symptoms of stricture were observed; and on receiving a hurt when practising gymnastics on the horizontal bar in December 1876, they became much aggravated. On admission his calls to urinate were very frequent, his stream forked and inclined to the left side. On examination a stricture was found in the bulbo-membranous portion of the urethra. A prolonged attempt to penetrate it was made with French flexible and slender silver catheters, but it was fruitless. A whalebone bougie with an angular point, graduated 1 to 5 (F. scale), was then passed down to the face of the stricture, and by gently twisting it from side to side its point soon became engaged in the orifice, but it required some little force to make it traverse the entire length of the contracted portion of the canal, the stricture being evidently a long and tortuous one. The bougie was tied in and left for forty-eight hours, when it was replaced by a No. 5 French catheter, which was allowed to remain for seventeen hours. I did not intend to continue the continuous dilatation for more than forty-eight hours; but on withdrawing the filiform bougie I found that the stricture had undergone but little change, and thought it better to again secure an instrument in the bladder. The second instrument slipped out while the patient was asleep, and instrumentation was not again resorted to for four days, as he complained of pain, and purulent discharge was abundant. After these symptoms had so far subsided dilatation was recommenced; but still only a comparatively small instrument could be sent through the stricture, so a No. 7 French was tied in for forty-eight hours, and on removing it a No. 12 French was passed easily. After this the progress was rapid, but does not require notice. I may here advert to the difficulty experienced in tying in very slender instruments, especially whalebone bougies, as the thread is apt to slip on account of the fineness and smoothness of the instrument employed. This difficulty presented itself in the case just mentioned, but was readily solved by Mr Noble, one of our students, who was present at the time. He suggested the use of a small pearl shirt-button, which answered perfectly, the end of the shaft being passed through one of the holes, and a thread of worsted attached to it. I think the suggestion was as ingenious as it was ready, and should not be lost sight of by those who may wish to tie in whalebone bougies.

Continuous dilatation was employed at first in all the cases now narrated; in two of them on account of the difficulty found in effecting an entrance, and in the remaining one on account of the narrowness of the stricture, and as progress would evidently have been tedious and unsatisfactory. It is not my usual custom to recommend the adoption of the continuous mode throughout, though I frequently have recourse to it as an adjunct in complicated or difficult cases. For, notwithstanding the favourable opinion expressed by many authorities as to the value of this mode of treatment, and though, as Mr Holmes remarks in his recent work on Surgery, "as much progress may often be made in the dilatation of a stricture in ten days by this method as would have been in a month by gradual dilatation," yet I cannot adopt this practice, nor can I accept the teaching of even so high an authority as Mr Savory, contained in the following sentences extracted from a paper contributed by him to St Bartholomew's Reports, vol. ix.:—"The mode of action of this plan of treatment contrasts favourably with that of any other; the stricture is overcome, the abnormal tissue is removed by constant pressure. . . . It acts not by a mechanical force, but by a physiological process. Of all modes of treatment this undoes the mischief in the most natural manner; and here, I submit, is its claim to be considered the best." Unfortunately, even in my own limited experience, it has fallen to my lot to treat cases which had before been subjected to treatment by continuous dilatation, where contraction had taken place to so great a degree as to make me doubt very much whether any absorption of the stricture-tissue had really ever taken place; and I believe that gradual dilatation is, in the great majority of cases, the mode most worthy of adoption; and that Sir Henry Thompson's maxim should never be lost sight of, viz.:—"First and foremost, dilatation—dilatation always—dilatation without exception, whenever it will succeed. It is always to be tried first, because it is the simplest and easiest mode."

## ARTICLE IV.—*Case in which a Knitting-Needle penetrated the Brain through the Orbit. Recovery, with Loss of Sight in one Eye.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 7*th February* 1877.)

ANNIE KELLY, æt. 14, was admitted into my ward in the Royal Infirmary on the evening of the 17th of January 1876. Two days before her admission she and some other girls were playing together in school, when she ran up against one of her companions who was knitting a stocking at the time, and as a result of the collision the point of one of the knitting-nedles passed through her upper eyelid and penetrated for some distance. The needle was at once drawn out, and the accident was immediately followed by pain in the head and loss of sight in the corresponding eye. No special head symptoms appeared for twenty-four hours after; but, at the end of this time, she became drowsy, restless, and had occasional delirium.

On admission she was somewhat drowsy, but answered questions when spoken to. There was general restlessness and delirium at intervals, and she complained of great pain in the left frontal and temporal regions. An examination of the left eye showed a small punctured wound about the centre of the upper eyelid. There was no discoloration around the wound, neither could any conjunctival ecchymosis or injury to the eyeball be detected. Both pupils were dilated, the left one being quite insensible to light. The sight of this same eye was entirely absent, and slight ptosis of the eyelid was noticed. When the patient was being put to bed it was also observed that she had a tendency to go round in a circle. Her temperature was 103°·4.

Active treatment was at once adopted, and, her head having been shaved, ice was applied to it. Her bowels were opened by enema, and twenty grains of bromide of potassium were given.

On the 18th her symptoms were much the same, but, in addition, she had occasional startings of the limbs, and the ptosis was more marked. Two leeches were applied to the left temple and gave much relief.

On the 19th she seemed decidedly better and had passed a quiet night. The left pupil remained quite insensible to light, and the sight of this eye was still absent. The sight of the right eye remained perfect. During the next few days the patient progressed favourably, but still suffered from restlessness and startings of the limbs.

On the 22d, Dr Argyll Robertson was kind enough to see her with me, but, owing to her condition, he was unable to make a satisfactory examination with the ophthalmoscope, and therefore could only confirm the correctness of the external symptoms which have been described.

On the 29th the patient was not so well. A rise in pulse and temperature took place, and she was more drowsy. Two small blisters were applied over the left temple.

On the 31st she was again better, and the pulse and temperature had fallen to the normal.

On the 2d of February the application of ice to the head, which had been continued since her admission, and all other treatment were discontinued, and she was allowed, for the first time, to have ordinary diet. The ptosis and all other symptoms had entirely disappeared, except the loss of sight in the left eye, which showed no improvement.

On the 24th the patient was dismissed.

One year after the accident I made inquiries in regard to the patient, and, finding she had gone into service at Glasgow, I asked Dr Hector Cameron of that town to examine her for me. He very kindly did so on the 27th of January of this year, and his report is as follows:—

"The vision of the right eye is perfect, and the eye itself quite healthy. With the left eye the patient can distinguish neither light nor shade. Examination of this eye with the ophthalmoscope shows a very marked and unmistakable lesion of the optic nerve, it being pale and atrophied, with great loss of substance. The disc was contracted and cupped; the vessels also were greatly contracted, and the lamina cribrosa could be distinctly seen."

*Remarks.*—I consider that this case is one of interest to the surgeon, medical jurist, and physiologist, more particularly as its history was well authenticated, and its progress and result carefully observed.

In its surgical aspect the case is an illustrative example of a penetrating wound of the cranium through the orbit; and, as such, I would first remark upon it. Similar cases of such an injury are on record in which sharp-pointed bodies have been thrust through the orbit into the brain. In a few instances of this accident recovery has taken place, but in the majority death has resulted from inflammation and suppuration in connexion with the brain and its membranes. A common history of these cases has been few or no symptoms for two or three days after the injury, and then head-symptoms have suddenly appeared and speedily proved fatal.[1]

From the nature of the instrument with which, and the manner in which, the injury was inflicted in the present case, and from experiments on the dead body, I am of opinion that the knitting-needle did not perforate the walls of the orbit, but that it passed through the optic foramen, lacerating the optic nerve and piercing, for a slight distance, the brain itself.

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[1] See Prescott Hewitt on Injuries of the Head, Holmes's System of Surgery, vol. ii.

That the optic nerve was directly injured is, I think, proved, judging from the immediate and persistent blindness of the left eye, and from the results of the careful ophthalmoscopic examination one year after the accident. The condition of this eye might be explained by the pressure of a clot on the nerve in the orbital cavity, inside the cranium or in the sheath of the nerve, as observed by Mr Prescott Hewitt, or by the results of inflammatory action; but, from the circumstances of the case, I am inclined to think that the nerve itself was injured.

In contrast to this permanent loss of nerve function, we have, in the temporary ptosis, an example of some interference with the action of the third nerve or its branches, consequent, in all likelihood, upon the presence of a blood-clot.

That the brain itself was punctured in this case I think most probable, from the symptoms which followed the injury, for they were such as are met with in traumatic inflammation resulting from cerebral injury. It is quite possible that the membranes only may have been injured and inflamed, or that the inflammatory condition may have spread from the injured optic nerve to them; but diagnosis between these conditions is not easy during the life of the patient, and is really not of much practical importance. The treatment adopted was that usually employed in traumatic inflammation of the contents of the cranium, and calls for no special notice. The satisfactory result of the case is, I think, explainable by the fineness of the body causing the injury, and also by the fact that the true nature of the case was from the first known, and in consequence early and actively treated.

To the medical jurist, and also to the surgeon, the case is important as an example of serious injury to the contents of the cranium with little or no external symptoms. The wound in the eyelid was so small, so free from surrounding ecchymosis or other discoloration, and the eye itself was so natural in appearance, that, unless the true history of the case had been surely ascertained, I do not think that its real nature would have been discovered; and the blindness and ptosis on the left side might have been referred to some other injury of the skull, or to some inflammatory condition of the brain or its membranes.

Cases are on record in which a penetrating wound through the orbit has not been discovered until after death, the body causing the accident having passed underneath the eyelid; and an accident still more likely to escape detection is that in which a body has been thrust through one or other nostril into the cranial cavity.

The point of a knitting-needle is not very sharp, and, in the present case, the needle was being held loosely in the hand; and this, I think, explains why it did not perforate the orbital wall. Having in the dead body thrust a similar needle through the upper eyelid at a point corresponding to the wound in the case reported, I found that its tendency—when not pushed in with any great force—was to pass along the roof of the orbit and enter either the sphenoidal fissure or optic foramen.

Physiologically, this case would have been of more importance if the exact extent of brain lesion could have been ascertained; but, as the case stands, it is, if my opinion be correct, an example of a direct injury to one optic nerve, external to the eyeball, resulting in atrophy of the nerve structures, and consequent permanent blindness of the corresponding eye without any other special change in the eyeball itself or in its external appearance, and without any interference with the functions of the opposite eye.

I am not prepared to say that the temporary symptoms of going round in a circle, noticed in this case, was the result of a lesion to any particular part of the brain. I am inclined to think that the lesion to the optic nerve is sufficient to account for this condition; and it is quite reasonable to suppose that the lesion to this nerve may have produced some temporary influence on the corpora quadrigemina, which have been referred to by physiologists as one of the brain points connected with this curious symptom.

## ARTICLE V.—*Case of Aneurism in Lateral Ventricle of Brain.*

By ALEXANDER ROBERTSON, M.D., Physician to the Town's Hospital and Asylum, Glasgow; and D. FOULIS, M.D., Pathologist to the Glasgow Royal Infirmary.

ANEURISMS of the size and in the situation of the one described in this article are of rare occurrence, though Dr Robertson met with one a number of years ago which resembled it both in appearance and position. In the present instance, however, there existed a very unusual condition of the bloodvessel from which the tumour springs, so that it has been thought desirable to put the case on record.

### REPORT OF THE CASE BY DR ROBERTSON.

*History.*—E. R., was admitted into the Town's Hospital on the 27th February 1865, being then eighteen years of age. She stated that her mother had suffered from palsy, but that her father and other relatives were healthy. About four years before admission she began to have "fits," which appear to have been epileptic in their character. Her whole body was convulsed while in them, and she sometimes bit her tongue if they were severe. They occurred in paroxysms at intervals of a month, six weeks, or two months. There might be three in a day on three or four days in succession at each paroxysm. For a day before the onset of the series she suffered from pain and giddy feeling in the head, beating at the heart, and a bad taste in the mouth. After these seizures had persisted about three and a half years she had a hemiplegic attack affecting the left side of the body. This was preceded for about a week by violent pain in the head, particularly above the eyes, a feeling of dizziness, vomiting, and a sensation of prickling in the side afterwards palsied. She did not lose consciousness at the time of its occurrence. Menstruation began at the same time as the "fits," but it has never been regular, there being sometimes an interval of two or even four months between the periods. She has not had rheumatism, nor is there any indication of syphilis.

*Condition on Admission.*—Patient can walk a little, but the "drag" of left leg is very decided; cannot move the toes, neither can she move the fingers, nor bend the wrist of the left hand, though she is able to flex and extend the forearm at the elbow. The sensation of these members, as tested by the æsthesiometer, is little if at all affected. Both arm and leg are wasted, particularly the arm, and the fingers cannot be fully extended without some force, owing to the contraction of the flexors of the forearm. The loss of power in the extremities was complete at first, and recovery to the existing amount has been very gradual. The contraction of the fingers did not begin till about three months since, and is thus a "late" symptom. There is a very distinct difference in the electro-muscular contractility of the two forearms: the muscles of the sound arm respond to a Faradic current which produces no effect on those of the palsied one, but the latter contracts under a current of greater strength. The left side of the face droops slightly, and sensation of that side seems impaired subjectively, as she complains of a feeling in it of what she calls "deaf nuts;" but testing in the usual way does not bring out any apparent difference between the two sides. The special senses are normal, and the pupils are equal and regular. There is not now any appreciable defect of speech, though when the palsy set in she spoke "thick," but could always make herself understood. The temperature in axilla of palsied arm is 97⅘, whereas in the sound one it is 98⅗, but to her sensation the palsied side is always the warmer of the two. With respect to her mind, she seems a little simple and facile, without any obvious impairment of mental power, but she admits that she now becomes more easily elated and depressed than before the paralytic seizure. The heart's action is more forcible than usual, but there is no increase of cardiac dulness, nor is there any murmur along with the sounds. Pulse 76, soft. Urine healthy.

*Progress and Result.*—Patient remained almost constantly in the ordinary wards of the establishment from her admission until her death, which occurred on the 25th November 1876. During these years she was generally able to walk about, and there seemed little or no change in her condition in any respect till the last three months of her life. Then, however, she began to get weaker and to complain occasionally of headache, and she seemed more exhausted than at an earlier period after the convulsive seizures, which recurred every few weeks, much as when she first came under observation. Towards the close the symptoms became more acute; the head got hot, and pain in it increased, the pulse quickened and there was delirium—all pointing to inflammatory action within the skull, and this soon terminated in death.

At the autopsy the brain-membranes were found injected, and at several points spotted with lymph; there was also considerable fluid effusion between the membranes. The tissue of the brain was rather anæmic. The further dissection of the brain proving unusually interesting, a committee of the Pathological and Clinical Society was appointed to carry it out in detail,[1] and the following remarks contain the substance of the report of this committee:—

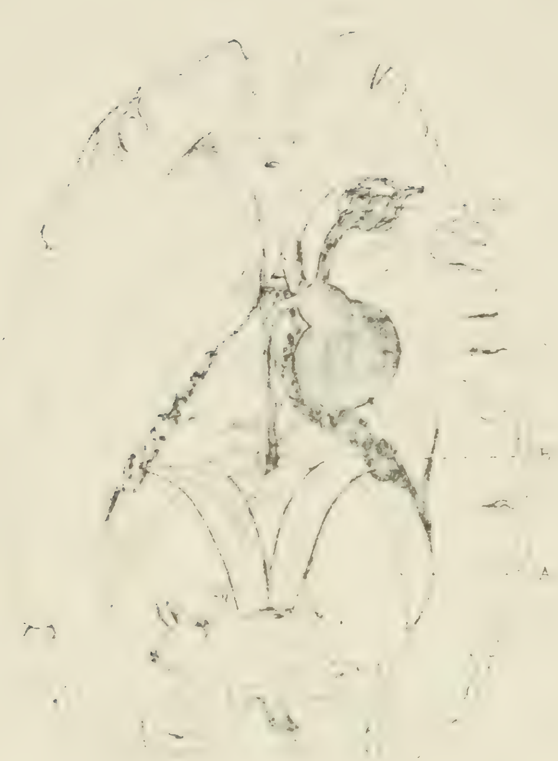
The right crus cerebri, and to a lesser extent the right half of the pons, were atrophied. Both lateral ventricles were enlarged, especially the right ventricle, and the convolutions and brain-tissue over the ventricles were rather diminished in thickness. In the substance of the right anterior lobe, just outside the cornu of the ventricle, lay a red stringy mass, which on more minute examination seemed to be a network of vessels, and in places to be cavernous tissue. This mass of tissue resembled to the eye a tassel, and was of the size of an almond. It communicated at both ends with vessels; at the outer end with a branch of the middle cerebral artery, which dipped in just above and in front of the island of Reil; and at the other end with a vessel of the size of a crow-quill, which ran curving backwards to the line between the corpus striatum and optic thalamus. Here the vessel was dilated at one side into a small aneurism of the size of a green pea; and from the side of the small aneurism sprang a second pouch as large as a walnut, round, smooth, firm, and filled with laminated clot. A fibrous capsule enclosed the larger pouch, continuous with the wall of the smaller one. In the small aneurism and at a spot near the upper part of the large one was fluid blood. The optic thalamus and the corpus striatum were much pressed upon and hollowed out by the larger aneurism, which made for itself a bed in these centres.

The vessel, after giving off the aneurisms, passed round to the foramen of Monro, which it entered side by side with the anterior end of the choroid plexus, with which, however, it did not seem to communicate. It then turned backwards and ended in the enlarged veins of Galen, enclosed in the velum interpositum.

### DESCRIPTION OF PLATE.

*a*. Fornix turned back.
*b*. Choroid plexus of right side.
*c*. Aneurism.
*d*. Tassel-like arrangement of bloodvessels in the substance of the anterior lobe—shown as if dissected out. Scale, ½.

[1] This committee consisted of Dr Robertson, Mr Clark, and Dr Foulis.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## ARTICLE VI.—*Case of Lead-Poisoning.* By MATTHEW JAMES TURNBULL, M.D., Coldstream.

ALTHOUGH extremely averse to follow the custom, which I think is much too prevalent in the present day, of publishing reports of cases which, however interesting to the practitioner in attendance, convey little information to the profession generally, I am induced to comply with the request which has been made to me by several of my medical friends, in whose judgment I have confidence, and to whose opinion I think it right to defer, to give the history of the following case, which I believe must be looked upon as one of lead-poisoning, resulting from the accidental swallowing of a lead bullet. I hope that it will not be considered out of place in me drawing attention to the subject of the possibility of metallic lead proving poisonous, since the belief that it is not so is very general. We have the high authority of Christison and Taylor for the assertion that all the preparations of lead are poisonous, except the metal. Professor Maclagan, to whom I am greatly indebted for much information, says that, as to the effects of metallic lead swallowed, the accounts are most discrepant. The general statement is that lead appears to exert no poisonous action when swallowed in the metallic state; but it does appear that metallic lead may be dissolved in the gastric juice, and do harm; only with regard to shot, bullets, etc., it must be borne in mind that they generally contain some arsenic, and that symptoms may be due to it. Mr Robertson, veterinary surgeon, of Kelso, who is justly esteemed as an excellent authority, informs me that metallic lead, when taken into the stomachs of ruminants, is capable of producing fatal results. Bullet spray from rifle butts has of late years been a common form in which metallic lead has reached the stomachs of cattle. The case I have to report is assuredly one of extreme rarity, and on this account alone appears worth recording. I have applied to very many authorities likely to be conversant with such matters, and, with the exception of Dr Langdon Down of London, they all say that they can find no record of any such case. Dr Down has kindly investigated the subject, and writes to me that the only case he can hear of was narrated by Pauline in one of the foreign journals about forty years ago. In the absence of that positive proof which a post-mortem examination alone could have afforded, I am fully sensible that I may be in error in attributing the fatal result to lead-poisoning. Whether or not my diagnosis was correct, may best be judged of by due consideration of the facts, which I shall narrate as exactly as I possibly can, as they were observed at the bedside.

About five o'clock on the morning of the 10th of January last I received a note from a gentleman residing nearly twelve miles off, begging of me to come at once to visit one of his sons, who he said had swallowed a lead bullet some days previously, and that, in consequence apparently of some obstruction, he was now seriously ill. I went immediately, and found the boy, who was nine years of age, in bed. He told me that he was still in pain, but that it was nothing now compared to what it had been before my arrival. The patient was more than usually intelligent, and, as I knew that I could rely upon the accuracy of his statements, I asked him just to tell his own story, which was very nearly as follows:—While playing at some games in the drawing-room on the afternoon of Friday the 5th, he accidentally swallowed a lead bullet which he had in his mouth. His mother, who was beside him at the time, at once gave him a dose of castor-oil, and told him to remain quiet (I subsequently learned that my friend Dr Dodds, of Yetholm, who had been in the house visiting one of the servants upon whom he was in attendance, was immediately recalled. He advised that he should just remain quiet, without any further treatment, as most probably no untoward results would follow. In a letter which Dr Dodds kindly wrote to me, he mentioned that, knowing of another similar case in which nothing was done, and where no ill effects followed, he did not apprehend any danger, and so little was thought of the circumstance, that, although I happened to be in the house on the evening of the 8th, his parents never even mentioned the subject to me.) He continued quite well up till the afternoon of Tuesday the 9th, when he complained of feeling ill and out of sorts, and went to bed, just about the same hour that he had swallowed the bullet four days previously. About ten o'clock the same evening he first felt pain in his bowels, which was relieved by the application of hot bran poultices, after which he fell asleep. About three o'clock on the morning of the 10th he awoke in great agony, and, seeing him so ill, his father at once sent for me. When I first saw him his pulse was 110 and his temperature 102°. On removing the bran poultice, which was applied to his bowels, he pointed out to me that "the awful pain" was just round the navel, and was like as if a burning hand inside was tearing his bowels back. Moderate pressure did not increase the pain, neither did the weight of the poultice, which was extremely hot, at all inconvenience him. Just as I had concluded my examination, another paroxysm of pain came on, and I shall not soon forget the poor boy's agony, as he well expressed it—it was indeed awful. His bowels, although previously confined, had been freely moved, and, although all the motions had been carefully examined, the bullet was not found. Up till this time I confess that I looked upon the distress as the result of some mechanical obstruction, and thinking that, from the form and size of a bullet, that it would probably soon pass through the bowels, I hoped that ere long all would be well. Very soon, however, another paroxysm of pain returned, and when I reflected upon its paroxysmal nature, its intensity, and its position, the conclusion was forced upon me that the case was not one of mechanical obstruction, but of lead colic, and this opinion I expressed. As soon as daylight appeared I examined the gums, but detected no evidence of any blue line; but, noticing that there was an entire absence of any tartar on the teeth, I did not consider that its being awanting justified my modifying my opinion. In addition to the continuous application of the hot poultices, which were very grateful, I ordered a dose of sulphate of magnesia, with dilute sulphuric acid and tincture of opium, and this draught to be repeated, unless the bowels were freely acted upon. Fifteen drops of dilute sulphuric acid in tepid water were to be given every eight hours. In the evening I received a telegram saying that the bowels had been moved, but that the attacks of severe pain had been very frequent. Soon afterwards a carriage arrived to convey me back as speedily as possible, the coachman being instructed to inform me that my patient was in great distress, especially from a constant desire to pass water, and inability to do it. Immediately on my arrival I gave him thirty minims of chlorodyne, after which he soon fell asleep, and passed a tolerably quiet night; so much so that, although occupying the next room, I was never disturbed. On visiting him in the morning he appeared better; the pain was not so severe, neither had the paroxysms been frequent; the tongue, although still a good deal furred, was moister; the pulse had fallen to 100, and the temperature to 101°. Believing that the important objects to be kept in view here were the relief of pain and the warding off of gastritis, while I endeavoured, by the repeated administration of dilute sulphuric acid, to keep the bullet coated with a neutral sulphate until it might become expelled, I directed him to have a pill containing a grain of opium every four hours, unless he was asleep; to have his abdomen freely rubbed with equal parts of chloroform and belladonna liniments, and, so long as they were grateful, to have the hot poultices re-applied. The diet to be chiefly pultaceous. Having thus far given an account of the symptoms, and of the principles which guided me in the treatment which I adopted, it would be alike tedious and unprofitable to continue a daily record of the details connected with the case; the most noteworthy facts were the repeated attacks of strangury and of severe pain in the thighs and knees. Although there were intervals of comparative relief from the abdominal pain, it was never entirely absent. When on my way to visit my patient on the 23d, I met some members of his family out driving, from whom I was pleased to learn that he was very much better. My first glance at him, however, filled me with disappointment, for, although he expressed himself much freer from pain than he had been hitherto, his appearance at once satisfied me that there was some change for the worse. His lips and tongue were blanched, his teeth were covered with sordes, and his breath had that peculiar smell so often noticed in patients suffering from malignant disease of the stomach. His pulse, which was small, had risen to 120; I was confident that there had been some internal hæmorrhage, and I felt constrained to express a very unfavourable opinion as to the result. On the following day his anæmic appearance was very marked, and prepared me to learn that he had vomited a considerable quantity of blood. He was extremely restless. Nothing would remain on his stomach but ice, and it was only for a short time that he could retain small quantities of Brand's essence of chicken and turtle-jelly. Pain was relieved first by enemata of starch and laudanum, and afterwards by morphia suppositories. Once, when he suffered much from tympanitis, a very hot turpentine epithem afforded him much relief. From this time onwards he became greatly emaciated, having more the appearance of a person who had been suffering for many months from some malignant disease, instead of from an illness of three weeks' duration. On the morning of the 30th I received a telegram saying that he was suffering great pain, especially in his knees, and that he was very anxious that I should come to him. When I arrived, I learned without surprise that he was dead.

If it does appear from this case that metallic lead may prove poisonous, I submit that, from a consideration of it, the following lessons may be learned:—

1st, Children should be discouraged from indulging in the by-no-means uncommon practice of swallowing lead shot.

2d, When consulted in cases where metallic lead has been swallowed, we should endeavour to have it evacuated by emetics, or otherwise.

3d, In such cases the prognosis should be more guarded.

Being actuated by only one motive in giving the facts connected with this case, viz., a desire to arrive as nearly as possible at the truth, I applied to Mr Robertson, of Kelso, to know if he could furnish me with any further information on the subject of the effects of lead upon cattle, and I have much pleasure in adding as an appendix the letter with which he has obligingly furnished me.

KELSO, 12*th March* 1877.

DEAR SIR,—I regard it no trouble, but rather a pleasure, if, by any facts which I can furnish, you may be helped to understand or elucidate any phenomena connected with the ingestion of metallic lead, and will gladly endeavour to answer the queries you put as briefly as possible.

In cattle the symptoms said to be attendant on the reception of metallic lead may be placed in two classes.

*A. Acute Lead-Poisoning.*—Saturnine epilepsy, or the "encephalopathic saturnine" of De Planches, marked by sudden loss of appetite, suspended rumination, occasionally colicky pains, constipation or violent diarrhœa, grinding of the teeth, collection of frothy saliva around the lips, visible mucous membrane sometimes injected. Almost invariably there is a great inclination to press the head against the wall, or some fixed body; spasmodic twitching of the muscles, particularly of the neck and head; strabismus, with eyes insensible to light; extremities cold; movements weak and tottering. Sometimes there is delirium or stupor, epileptiform convulsions, and coma, these latter becoming more pronounced as a fatal termination approaches.

*B. Plumbism—Chronic Lead-Poisoning.*—Malaise, with loss of flesh for some months, gradually impaired appetite, withdrawing themselves from the herd, a retracted and rigid condition of the abdominal walls, staring eyes, dull sleepy look, and somewhat staggering gait, frequent stretching of themselves and groaning heavily, bowels sometimes constipated, at others diarrhœic. In some cases the blue line where the gums join the teeth is distinct enough, at others does not exist.

The time given between the ingestion of the metal and the development of symptoms of illness, in those cases spoken of as acute, is not exactly stated.

The material believed to have acted thus fatally was sheet-lead from tea-chests; it had been spread along with some top-dressing on ryegrass and clover in November, and the cows were placed in the grass in the spring following, date not given. The first fatal case occurred on the 17th May, so that, in all probability, the metal could not have been in the animals' stomachs many weeks.

In the other cases spoken of—chronic lead-poisoning—the form in which the metal was ingested being bullet-spray, the periods given are from twenty-eight to fifty-four weeks.

The morbid appearances noted in post-mortem examination vary somewhat in these two classes of cases.

In those animals believed to have died from partaking of the tea sheet-lead, this material was found in great amount—16 oz. in the abomasum, the true digestive stomach. This viscus contained fluid of a dark colour. Mucous membrane not *hyperæmic*, rather the opposite, and somewhat thickened. Intestines distended with gas, dark-coloured ingesta, patches of ecchymosis, liver pale but firm, kidneys natural, gall and bladder full of dark-coloured bile, pericardium ecchymosed, and contained bloody-coloured serum. The masticated sheet-lead coated with a gray-white deposit.

In cases of chronic lead-poisoning from reception of metallic lead, there has been observed much straw-coloured fluid in the abdominal cavity; the mucous membrane of the stomach showing patchy inflammation, lungs congested, the intestines, large and small, of a peculiar blue appearance, liver paler than natural, heart natural, coagulum in both sides. Much bullet-spray in second and fourth compartment of stomach. This material coated with dirty-white incrustation, which proved, on analysis, to be carbonate of lead.

Lead found on analysis in coats of stomach and intestines, also in liver, spleen, blood, etc.

The cases which terminated fatally from swallowing tea sheet-lead lived from four to eight days.

Those animals believed to have been poisoned by the ingestion of bullet-spray were ill for several months, several being under observation and treatment for seven months.

The cases which are believed to have succumbed from the reception of tea sheet-lead are remarkable, as pointing in an opposite direction to the generally-entertained idea that metallic lead requires a very lengthened period of retention in the stomach ere the gastric and other fluids are capable of producing a soluble and poisonous compound.

The others, where bullet-spray is believed to have been the offending agent, are, I believe, not uncommon, and are generally recognised and accepted—apart from the cases which occur in lead-smelting districts—as examples of one of the commonest forms of lead-poisoning met with in the lower animals.—I am, yours truly,

WM. ROBERTSON.

## ARTICLE VII.—*Experimental Researches on the Action and Sounds of the Heart.* By GEORGE PATON, M.D.

As a general proposition, the profession maintain that the first sound of the heart is synchronous with the ventricular systole, and the second sound synchronous with the ventricular diastole. They admit that the aorta is elastic, and reacts with a force equal to that by which it is distended; and that the blood is propelled in successive waves along the arteries to the extremities of the body.

If we observe the action of the denuded heart in a living animal when it is maintained with vigour, we perceive that two functions are engaged in maintaining the circulation of the blood: 1*st*, The auricles receive the blood on its return to the heart, and transmit it to the ventricles. 2*d*, The ventricle and aorta act as dischargers of the blood, impelling the blood-wave along the arteries to the extremities of the system, and the statements applicable to one side of the heart equally refer to the other, for, from the manner in which the fibres are interlaced, the ventricles act simultaneously. The contraction of the auricles is also simultaneous, and, when the action is vigorous, alternate with that of the ventricles.

We consider that, if we fully understand the manner in which the action of the heart is maintained, we will more correctly comprehend the cause of the sounds of the heart.

If the ventricle propelled a wave of *fluid* along a rigid or unyielding tube already filled with fluid, it would at every contraction displace an amount of fluid equal to that which it forces into the tube, viz., two ounces; a similar quantum being discharged at the further extremity. And the valves placed at the base of the tube would close the orifice as the ventricle commenced to dilate, the blood falling back and closing the valves on the propelling force ceasing. In this case the valves are closed in connexion with the diastole of the ventricle.

But when the ventricle propels the blood-wave along the aorta, which is an *elastic tube*, in displacing the blood which the aorta contains, it distends the walls of the aorta; and from the energy of the contraction, the ventricle persists contracted at the orifice, sustaining the impulse, whilst the aorta reacts, closing the valves, and completes the wave. In this case the valves are closed in connexion with the ventricular systole, and the sound produced is a systolic sound.

And, as a general law in propelling a wave of fluid along an elastic tube, or a tube strongly elastic at the base, the walls yield as the fluid enters, and are distended. But the wave being forced in, we maintain a momentary pressure at the orifice till the walls react, and in reacting they close the valves and complete the wave.

There are then two impulses imparted in propelling a wave of fluid along an *elastic tube*, as the distended walls must react, closing the valves, to produce the full wave. And in the action of the heart these two impulses are united or combined.

When the ventricle propels the blood-wave into the aorta, distending its walls, it does not merely persist contracted whilst the aorta reacts. But the ventricle, on being fully contracted at the base of the distended aorta, to which it is attached, shortened in its longitudinal axis, exerts a tension or slight retraction on the base, that produces the simultaneous reaction of the aorta, closing the valves, and completing the wave at once.

This is the manner in which the action of the heart is maintained in every class of vertebrated animals in which we have examined it when denuded. The base of the distended aorta being slightly drawn down by the contracted ventricle, and retained tense as the aorta reacts and completes the wave, or, as it may be expressed, produces the full blood-wave along the arteries.

The heart having just been exposed in a living animal, and its action strong and vigorous, we perceive that the ventricle acts with a sort of back stroke, forcing, as it contracts, the blood-wave into the aorta; and on being fully contracted, shortened in its fibres, it exerts a strain or slight retraction on the distended aorta that secures its simultaneous reaction—the ventricle persisting contracted whilst the aortic impulse produces the full blood-wave.

But physicians believe that the contraction of the ventricle *per se* propels the blood-wave along the aorta and arteries to their termination—a view opposed to the physical law that we have just stated, that obtains in the propulsion of fluids along an *elastic tube;* and limiting in that case the first sound of the heart to the contraction of the ventricle. And they believe that, as the ventricle dilates, the blood in the aorta falls back against the sigmoid valves, and closes them, producing the second sound of the heart. Bucquoy, a recent French author, says:—"During the ventricular diastole, the cavity, having been nearly effaced, takes suddenly its first dimensions, and the blood would then flow into it by the auriculo-ventricular and arterial orifices, if the latter did not present, by the sudden lowering of the sigmoid valves, an obstacle to the return of the blood from the artery into the ventricle." Dr Williams states (p. 213):—"The second sound being caused solely by the sudden reaction of the arterial columns of blood on the semilunar valves, its loudness will depend on the mobile and perfect state of these valves, and the extent and abruptness with which they are stretched by the recoil of the blood at the moment of the ventricular diastole." Dr John Reid considered that "the blood in the large arteries, by their elastic power, was forced back against the semilunar valves, closing them during the ventricular diastole." In all these cases the blood is spoken of as falling back or being forced back against the sigmoid valves, and closing them during the diastole of the ventricle. But if there be one fact in the action of the heart clearly established by observation and experiment, it is, that the distended aorta on reacting forces the blood against the sigmoid valves, and closes them in completing the wave, or in producing the full wave along the arteries; the blood-wave being distinctly seen to pass along the aorta as the sigmoid valves close; and the ventricle to be still contracted, firm and hard in its walls, exerting a tension on the base of the aorta as the aorta reacts. The fibres of the ventricle do not relax till the aortic impulse is imparted to the wave; and as the two impulses take place simultaneously with the ventricular systole, the sounds produced unite and constitute one prolonged systolic sound—the first sound of the heart.

We have distinctly determined, in the course of our experiments on the alligator terrapin, that the full blood-wave is produced on the reaction of the aorta, and that the first or prolonged sound of the heart terminates as the aortic valves close; the sound being caused by the force with which the ventricle propelled the blood through the aortic foramen, and the valves closed by the blood recoiling against them as the aortic impulse produced the full wave, the ventricle being still contracted, exerting a tension on the aorta. The temperature of the day being high, and the action of the heart strong and vigorous, beating at the rate of 36 or 40 pulsations per minute, we could recognise the first sound, not only through the medium of the stethoscope, but by the naked ear applied over the part, and nothing to interfere with the heart's action. The sound was most distinctly heard at the origin of the aorta over the aortic valves, or a little higher up the aorta, and nothing could be more satisfactory than the clear and distinct manner in which the sound was produced.

Closely following the first sound, the second sound of the heart was heard—a short, sharp, and acute sound, produced by contraction of the auricles as they sent the blood with force through the auriculo-ventricular foramen into the ventricle.

The fact that the aortic valves are closed simultaneously with the ventricular systole, and not with its diastole, is further proved by the tracings of the cardiograph.

This figure or diagram represents the movement of the right ventricle in tracings of the cardiograph. A represents the contraction of the ventricle. *a* represents the time the ventricle persists contracted[1] before the diastole B commences; the ventricular systole extending from the base of A to B, which represents the line of descent or diastole; and the plateau or horizontal line *a* represents the time of the aortic reaction, because, in the tracings of the sphygmograph,[2] the plateau or horizontal line disappears in incompetency of the aortic valves, and is converted into a sort of crotchet passing into the diastole. Proving that, in the healthy and vigorous action of the heart, the aorta reacts and closes the valves as the plateau commences, whilst the ventricle is still contracted, and before the diastole occurs—a fact in accordance with all that we have observed in the course of our experiments—the ventricle retaining its tension on the aorta, and not relaxing till the aortic impulse is imparted to the wave; it being by a ventriculo-aortic impulse that the full blood-wave is produced along the arteries.

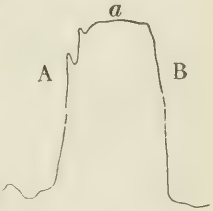

FIG 1.

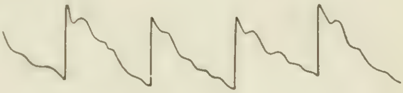

FIG. 2.

If the aorta forced the blood against the valves and closed them during the diastole of the ventricle, as its walls were soft, flaccid, and relaxed, it would tend to stretch the tendinous ring at the base of the aorta, and render the valves incompetent. The aorta must be supported at its base, held firm and tense when its walls react and close the valves—a provision which has been particularly attended to in the structure and action of the heart. No doubt the tendinous zones at the base of the aorta render it less distensible than the middle coat of the arteries, but it could not withstand the constant reaction of the distended aorta, forcing the blood against the valves as it produced the full blood-wave, unless supported by the contracted ventricle.

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[1] The ventricular systole persists a certain time after the line of ascent has attained the summit, as the well-defined plateau or horizontal line *a* shows that precedes the line of descent.—Bucquoy, "Sur les maladies du cœur," p. 17.

[2] Figure 2 represents the tracings of the pulse by the sphygmograph in incompetency of the aortic valves.

"Les particularités que présente ce tracé sont les suivantes : 1°, Une ligne d'ascension assez élevé et absolument vertical ; 2°, Le plateau remplacé au sommet de cette ligne par un angle très-aigu formant une sorte de crochet ; 3°, La déclination rapide de ligne de descente."—Bucquoy, "Sur les maladies du cœur," p. 71.

But we are told that, in experiments on the denuded heart of the ass, the second sound of the heart was heard on the closing of the sigmoid valves, and when that was prevented, the sound was converted into a hissing. But it is impossible that this could be the second sound of the heart synchronous with the diastole of the ventricle, for the ventricular systole was not then completed; these valves were closing to produce the full blood-wave along the arteries. The fact is, these physicians were not acquainted with the manner in which the action of the heart is maintained; they were not aware that the ventricle persists contracted, sustaining the base of the elastic aorta when the aorta reacts, closing the valves, and completing the wave, and that the sound produced in closing the valves must be a systolic sound.

We cannot separate the contraction of the ventricle and the action of the aorta in producing the full blood-wave; they must be combined, and the sounds produced constitute one prolonged systolic sound—the first sound of the heart.

We thus perceive that the integrity of the aortic valves is indispensable to the completion of the full blood-wave, and to the first or prolonged sound of the heart.

If the aortic valves become incompetent the *full blood-wave* is not produced, and we have the pulse of unfilled arteries; and the first sound of the heart is rendered short, that portion of the sound connected with the closing of the valves being converted into a murmur, termed the aortic regurgitant murmur. But this is not the second sound of the heart heard on applying our ear to the chest, or in listening to the sounds of the denuded heart in a living animal. The action has now become morbid—the pump, so to speak, is disabled in some of its valves, and cannot produce a full wave along the tube. And not only is the wave imperfect, but a portion is forced back through the incompetent valves at every reaction of the distended aorta, opening up the ventricle before the diastole usually occurs; and the ventricle being in this abnormal manner partially dilated before the auricles contract, their contraction, from the limited amount of blood that they expel, produces a weak or indistinct sound. And as the blood does not arrive in sufficient quantities at the extremities of the body to sustain the nutrition of the system, the patient looks pale and exsanguine, and the stress of the circulation being thrown upon the contraction of the ventricle, it labours to sustain it by increased action, leading to dilatation and hypertrophy.

The fact, then, of an aortic regurgitant murmur occurring in connexion with the abnormal diastole of the ventricle does not prove that the second sound of the heart in its normal action depends on the closing of the aortic valves, for in the healthy condition of these valves the murmur does not obtain, but the full blood-wave and prolonged sound of the heart is produced; and were it possible to restore the integrity of the valves, the first sound of the heart would terminate as these valves closed.

We must then look to another source for the cause of the second sound of the heart, and that is contraction of the auricles.

We have distinctly found in the course of our experiments that, in the vigorous action of the heart, the auricles contract immediately after the aortic reaction is produced, and the sound caused by the force with which they send the blood through the auriculo-ventricular foramen constitutes the second sound of the heart. The auricles have been refilled during the contraction of the ventricle, and the moment the ventricle commences to dilate, the auricles contract; the walls of the ventricle relaxing as the blood enters the cavity, bringing it to a state of distension, the contraction of the auricles being simultaneous with the ventricular diastole, and the second sound of the heart immediately succeeding the first.

Laennec correctly attributed this sound to the contraction of the auricles, and Herard states, that in constriction of the mitral orifice, the auricular systole may even produce a bruit, not presystolic, but simultaneous with the "second temps" or diastole of the ventricle, and refers us to stethoscopic signs of constriction of the mitral orifice, and specially of a "bruit de soufflet" with the "second temps," or diastole of the ventricle, showing that, in the vigorous action of the heart the contraction of the auricles is simultaneous with the ventricular diastole. And physiologists are beginning to admit the fact, that in the active state of the circulation the contraction of the auricles immediately succeeds the ventricular systole.

But a difficulty has here occurred to physicians, from the fact that, in their experiments on the action of the heart, the contraction of the auricles appeared more immediately to precede the contraction of the ventricles, which they could not reconcile with the second sound. But this difficulty admits of an easy explanation, for the action of the heart is adapted either to a quick or slow circulation of the blood, and the auricles are the excitors of the contraction of the ventricles. If the action be quick and vigorous, the auricles contract immediately after the ventricular systole is produced, and the second sound of the heart closely follows the first. If the action be slow or weak the ventricle becomes partially dilated with blood from the auricles before they contract, and their contraction being limited produces a less distinct sound, which more immediately precedes the contraction of the ventricle. And in irregular action of the heart, as in advanced disease of the mitral orifice,—in incompetency and constriction, the auricle may be rendered so weak from over-distension, that the sound produced in contraction is converted into a "bourdonnement" or weak murmur, which immediately precedes the ventricular systole.

But in the vigorous action of the heart, the short and acute sound heard on applying our ear to the chest, at a point a little lower than is usually assigned to us, depends on the auricular contraction.

We therefore conclude, that the first or prolonged sound of the heart depends on contraction of the ventricles and closure of the sigmoid valves; and the second, or short and acute sound, that immediately follows the first, is produced by contraction of the auricles.

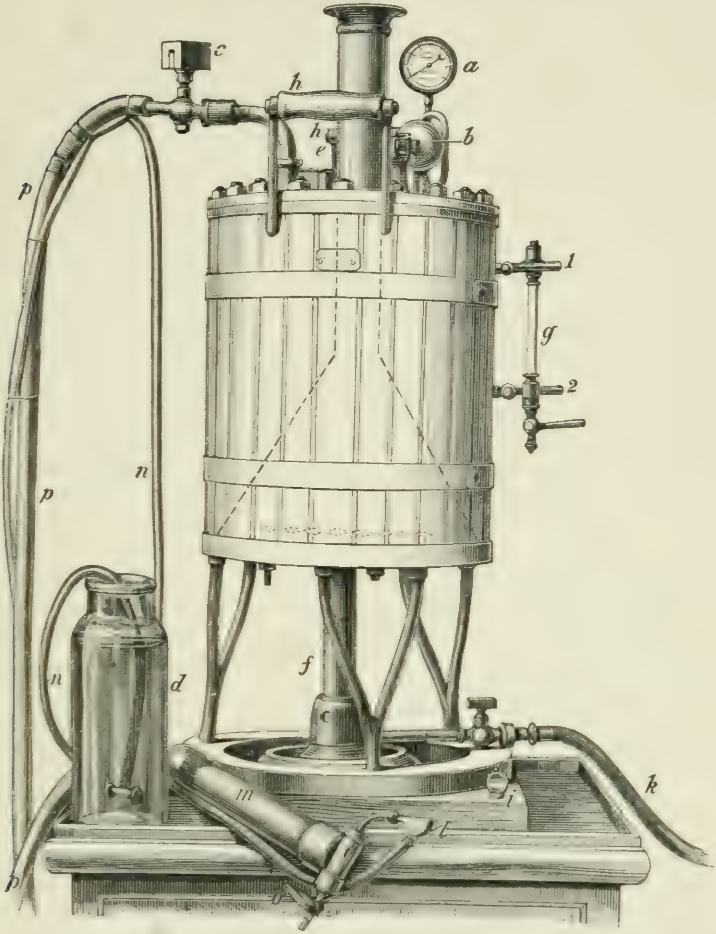
## ARTICLE VIII.—*A Steam-Spray Producer for Hospital Use.* By R. A. GIBBONS, M.B., East Suffolk Hospital, Ipswich.

THE accompanying drawing is an illustration of a steam-spray apparatus used in this hospital, which I had made with the kind permission of the surgical staff, under the direction of Mr H. Powles, of Messrs E. R. & F. Turner, engineers, Ipswich, and I have to thank Mr Branford Edwards for the interest he has taken in it, and for his many useful suggestions.

The small spray-producer we had in use formerly constantly failed, after it had been working a very short time. This is partly on account of the small size of the boiler, the supply of steam becoming so rapidly exhausted that there is not sufficient pressure to suck up any carbolic acid; and partly because, owing to the use of an ordinary gas-burner, the bottom soon gets coated with soot, which conducts heat badly, and prevents the steam from being properly kept up. Latterly, before using the present instrument, spirit was almost entirely used instead of gas.

I thought that if we had a large boiler there would be no fear of the pressure failing, and that it might be possible to have a spray at the end of long pipes, the antiseptic fluid being drawn from a distance. This is perfectly successful; the steam rushes past the orifice of the pipe for the acid, causing a vacuum, the carbolic acid takes its place, and a spray results.

The advantages are—*first*, that, owing to the size of the boiler, there is a continuous pressure, avoiding all possibility of the spray getting smaller or altogether failing; *second*, the one directing the spray has merely a small light handle in his hand, and can move from bed to bed without the fatigue of holding a heavy instrument, which is a consideration of some moment. Having the spray-producer sliding on a rod, so that it can be kept at any height, is open to the objection that it is then fixed, and cannot follow any movement the patient might make; *third*, and a matter of great consequence, it enables many operations to be performed, or cases dressed, without the extreme annoyance of having to wait, perhaps in the middle of an operation, for the boiler to be replenished.



*a*. Pressure-gauge for showing the pressure of steam in the boiler.
*b*. Safety-valve.
*c*. The tap for turning on the steam, which passes down the pipe *p* to the spray-point.
*d*. Bottle for antiseptic fluid.
*e*. A brass plug which is taken out by unscrewing when the boiler requires filling.
*f* Bunsen gas-burner.
*g*. Glass water-gauge for showing the water-level in the boiler; the handles 1, 2, are for shutting off communication with the boiler in case of the glass breaking.
*h.h.* Handles for lifting the boiler
*i*. Screw for fastening the boiler to the carriage.
*k* Gas-pipe.
*l*. The spray-points.
*m*. The wooden handle through which the steam-pipe passes.
*n*. Tube from carbolic bottle.
*o*. The tap attached to the little cylindrical box, *t*, for discharging condensed water from the steam-pipe.

The boiler is made of copper, and in such a way, as will be seen from the drawing, that the largest amount of surface is given to the heat; it is covered externally with wood to prevent loss of heat by radiation, and the top is screwed on with nuts, so that it can be easily taken off and the inside cleaned. It holds nearly nine pints of water, although so much need never be put in at once, and a beautiful spray may be kept up continuously for about two hours.

The water-gauge and pressure-gauge are necessary and very convenient. The pressure can be regulated by the flame of the gas-burner, which is a large Bunsen, suggested by Mr Edwards, and is a great improvement upon the ordinary burner.

The steam blows off at a pressure of 20 lbs. to the square inch; this is quite heavy enough for the safety-valve, as it is unnecessary to work with a higher pressure than 12 lbs. or 15 lbs. to start with. It is better to begin with this, because the steam-pipe being cold, a certain amount of steam, which at first condenses as rapidly as it enters the tube, is required to heat it. After the spray has been going a short time about 8 lbs. is quite enough.

The tubes for the steam and the antiseptic fluid may be any length; but, when too long, they are cumbersome, and a higher pressure is required. The former is made of canvas and indiarubber; the latter is an ordinary small indiarubber tube. The length of the ones used here is twelve feet. In the drawing at *t* will be seen a little cylindrical box with a tap,—an excellent contrivance of Mr Powles, to catch the condensed water. While the spray is going this tap should be opened occasionally to let the water, which is always forming to a certain extent, blow out; otherwise, it will be forced through the steam-point and make the spray unpleasant. Opening this in no way interferes with the spray, as there is always plenty of steam to pass out at the spray-point and tap at the same time.

The boiler is fixed to a little carriage, and can be wheeled with ease from one ward to another; if it has to be taken to a lower floor, it may be lifted from the stand and carried by the handles.

It may be thought that this apparatus is troublesome and difficult to work; but it is really not so, and, after using it every day for now nearly a year, remembering my experience of other sprays, I am convinced of its advantages. It will be seen that, with the exception of the fluid being drawn from a distance, it is the same instrument as that of Professor Lister, only enlarged. It is, of course, only intended for hospital use, where there is always plenty of dressing to be done.

I was asked to publish this, and, as I believe and trust, antiseptic surgery is now more extensively practised than formerly, I do so with the hope that the instrument, which is of so much service here, may prove as useful to other institutions.

The cost of the whole apparatus is about £15.

Since writing the above, I have had made for the acid a bottle with a stop-cock at the bottom; the tube for the carbolic acid is attached to the stop-cock, and is at once filled with fluid by turning the tap.

## ARTICLE IX.—*On the Therapeutics of Aloin.* By WILLIAM CRAIG, M.D., F.R.S.E., F.R.C.S. Ed., etc., Lecturer on Materia Medica, Edinburgh School of Medicine.

(*Read before the Medico-Chirurgical Society of Edinburgh, 7th February* 1877.)

ALOES, the *aloe* of the Pharmacopœia, is the inspissated juice of the leaves of various species of Aloe. Although it has been used in medicine for upwards of two thousand years, our knowledge of the chemistry of this drug is still imperfect. As yet only two bodies have been isolated, and both of these have been discovered by the Messrs Smith of this city. In 1851 they discovered in Barbadoes aloes a crystalline body, to which they gave the name *aloin*, because by them it was supposed to be the active principle of the drug. Besides aloin, aloes contains a volatile oil, to which the drug owes its peculiar flavour. This oil was first isolated by the Messrs Smith in 1872. It is a pale yellow, mobile liquid, with a resemblance in taste and smell to the oil of peppermint. It differs from the oil of peppermint both in its specific gravity and in its boiling point. It exists in small quantity in aloes, only one fluid ounce being obtained from 400 lbs. of aloes. Aloes also contains a considerable amount of a substance called *resin of aloes*. This resin deposits from a decoction of aloes as it cools. It is partially soluble in rectified spirit, about 85 per cent. being dissolved by spirit. Resin of aloes leaves, when burnt, a considerable amount of ash. These are the most important constituents of aloes. It is to the first of these constituents that I mean specially to direct your attention; but, as the resin of aloes generally contains a variable amount of aloin, I wish to make a few remarks on the action of this resin. In many works on materia medica this resin is spoken of as *modified aloin*, and consequently partaking more or less of the actions of that substance. By others it is spoken of as a very active substance, if not the active principle of aloes. Others, of whom I am one, believe that it is wholly inert. Chemically, it is very different from aloin, inasmuch as it leaves a considerable amount of ash when burnt, whereas aloin leaves no ash. Physiologically it is also very different from aloin. It was long supposed that the resin was the cause of the griping of aloes. The experiments of Dr Farre, Dr Garrod, and Dr Harley, have shown this theory to be utterly untenable; and in a series of experiments I performed in 1874, I was enabled fully to confirm their opinion. I found that even in doses of twelve grains it failed to produce griping. Unless this resin is prepared with extreme care, it will contain an amount of aloin. This, I believe, to a great extent accounts for the difference of opinion regarding the activity of this substance. I performed, three years ago, a series of experiments with this substance upon rabbits, with the view of ascertaining the action of the resin of aloes, and afterwards confirmed my conclusions by experiments on the human subject; and the conclusion to which I came is thus expressed in the *Edinburgh Medical Journal* for June 1875:—"The resin of aloes, when thoroughly exhausted of aloin, possesses no purgative properties." I still adhere to that opinion. In one of the best manuals of materia medica in this country, and one of the most recent, I mean the very excellent edition of Royle's Materia Medica by Dr Harley of London, reference is made to my paper on this subject, and Dr Harley states that his observations do not support my statement, inasmuch as he found that two grains of resin mixed with three grains of soap purged him freely. It might be a fair question for discussion, what effect the soap had in producing this purgation. In experimenting with cathartic substances we should never use soap, which is itself laxative. Gentian would be a far better substance to use in such cases. I have already mentioned that it is extremely difficult entirely to exhaust the resin of aloin, and I have the authority of Mr Smith to say that the resin of aloes with which Dr Harley experimented, and which was supplied by the Messrs Smith, did contain aloin. In my first experiments with the resin of aloin I obtained similar results, and on communicating these results to Mr Smith, he immediately examined the resin, and found that it contained a small amount of aloin, and afterwards prepared some which he believed to be as nearly exhausted of aloin as resin can well be; and when I experimented with this resin, I found it perfectly inert, even in doses of twelve grains. The Messrs Smith have since supplied Dr Harley with this carefully prepared resin, but what is the result of Dr Harley's experiments with this resin I am not aware. Dr Farre of London gave to patients doses of three grains of resin of aloes, and says that he found it almost inert, and that it seldom produced any evacuation. I believe that any little purgative effect which he observed might have been due to the aloin it contained.

I come now to speak more at length regarding *aloin*. This I believe to be the active principle of aloes. Aloin exists in all kinds of aloes. Good Barbadoes aloes contains about 25 per cent. of aloin. Aloin occurs in long yellow acicular crystals, transparent and doubly refracting. It is sparingly soluble in cold water, but freely in ether and chloroform. It is an oxidized hydrocarbon, having the formula $C_{16}H_{18}O_7$, according to some chemists, differing, however, in different species of aloes in the amount of water it contains. According to Dr Tilden, the aloin obtained from Barbadoes aloes contains three atoms of water; that from Socotrine aloes, one atom of water; whilst that from Natal aloes is anhydrous. These various kinds of aloes differ also in their solubility and form of their crystals, but are believed to have the same physiological actions. They are generally distinguished as Barbaloin, Socaloin, and Nataloin, according as they are obtained from Barbadoes, Socotra, or Natal aloes respectively.

Aloin is the only active principle contained in aloes, and possesses all the properties of the crude drug, and is free from griping. In small doses it is a tonic. It assists digestion. It gives tone to muscular tissue, and is thought to exert a special influence on the liver. In larger doses it is a purgative, acting specially on the large intestine. It increases the intestinal secretions, and also the peristaltic action of the bowels. It is believed also to stimulate the uterus, and hence is useful in certain sluggish states of that organ. The sulphate of iron, and other substances, increase its purgative action. I have experimented largely with aloin on rabbits, and have always found it an active aperient, and have given it almost daily to patients for several years, and have found it a trustworthy and beneficial medicine. Although aloin has now been used in medicine more or less for a quarter of a century, nevertheless there are still held most erroneous views regarding its actions and uses. Even so recent as February 1876, we find an anonymous writer in the *Edinburgh Medical Journal* thus writing regarding aloin:—"Dr Craig insists strongly on the superior efficacy of aloin over aloes in doses of ½ to one grain, the dose of aloes being stated as two to six grains; but every practitioner is aware that, for most purposes, a much smaller dose is sufficient, ½ to one grain generally fulfilling all its indications. Hence it is very doubtful if aloin is more powerful or even equal to good Barbadoes aloes." The above was evidently written by one who was ignorant of the very first principles of therapeutics, and he has been led to draw a most illogical conclusion from false premises, showing that his logic was as deficient as was his knowledge of drugs. This unknown author accuses me of stating the dose of aloes as two to six grains—that being the dose given in the British Pharmacopœia and in all the best works on materia medica—and what I am bold to affirm is the correct dose of aloes. The merest tyro in therapeutics knows that there are instances in which smaller doses than those given in the British Pharmacopœia will suffice for our patients; but no sane person ever argued from this that the doses given in the British Pharmacopœia are incorrectly stated. Every practitioner knows that constipation, the disease for which aloes is most frequently given, may exist in any degree, from the merest tendency to costiveness up to complete stoppage of the bowels; and every practitioner knows that the quantity of medicine will vary accordingly. It is quite true that less than two grains of aloes will frequently suffice, but it is equally true that in such cases less than ½ grain of aloin will be sufficient. In such cases it will be found that ⅛ to ¼ grain of aloin will be all that is required. The best authorities have come to the conclusion that aloin is at least two or three times as active as good aloes.

Mr Finlay Dun, in his excellent work on Veterinary Medicines, states as the result of his experiments, that two drachms of aloin acted on "horses several hours earlier, without impairment of appetite or spirits, and with the certainty and effect which usually follow six drachms of Barbadoes aloes." He also mentions that Mr Dollar of London, as the result of his experiments, "concludes that, comparing aloin with the crude drug, little less than half the quantity acts in horses with more certainty and equal effect." Dr Harley of London states, in the last edition of Royle's Materia Medica, that "the action of aloin is at least twice as strong as that of the best aloes." He further states that, as the result of several experiments, he has come to the following conclusions:—

"1. The cathartic action was uniform, rather more speedy than that of crude aloes, and unattended by griping.

"2. The average dose for a strong adult male is about 1½ grain, which usually causes two or three copious evacuations. 2½ grains were always followed by powerful catharsis, producing five or six evacuations."

Even Dr Tilden of London, a chemist who has devoted much attention to the constituents of aloes, is rapidly advancing in his views regarding the activity of aloin. So late as 1870, we find Dr Tilden maintaining that "the purgative property of aloes is not due to aloin, and that the active constituents of aloes is still unknown." In 1876, however, we find him stating that "Barbaloin in doses under two grains frequently produces some slight laxative action. Two grains cause a decided effect, never, however, amounting to complete purgation." He further admits that aloin causes less griping than aloes. Dr Tilden, in conjunction with Mr Dobson, F.R.C.S., performed some experiments with aloin in the Bristol Hospital, and their observations have been recorded in the *Medical Times and Gazette*, 12th August 1876, and in the *Pharmaceutical Journal* for 19th August 1876. They experimented in fifty cases, mostly adult males, and they state that they never gave Barbaloin in more than two-grain doses. They have clearly demonstrated that aloin is possessed of aperient properties, and that it causes less griping than the crude drug. After quoting from my paper on aloin in the *Edinburgh Medical Journal* for June 1875, they state that, "so far as their observations go, they do not fully agree with the experience of Dr Craig." Possibly this may, to some extent, be accounted for by the fact that they had not tried it in a sufficient number of cases; or, at all events, in cases suitable for the administration of aloes. It appears to me that some of their own conclusions are altogether untenable, and are inconsistent with other parts of their paper. Be it borne in mind that in no case did they give more than two grains of Barbaloin, and contrast this with the fact, that they state in another portion of the paper that "it did not appear to them that the crystalline principle was more potent than a similar dose of aloes." What would we say of conclusions drawn regarding the action of Barbadoes aloes if no dose higher than two grains had been given? Is not the very fact that they did not give Barbaloin in larger doses than two grains, to prove its purgative properties, an indication that they believed it more active than Barbadoes aloes?

That aloin possesses purgative properties there cannot be the least shadow of a doubt. I am not aware of any good authority who denies this; and, as the results of my experiments on rabbits, I have come to the conclusion that it is the only active principle contained in aloes. I have, during the past three years, prescribed aloin almost daily, and have always found it a reliable and valuable medicine. It is chiefly in chronic constipation that I prescribe aloin. In habitual constipation it is one of the very best medicines we possess. To persons of a sedentary occupation, with constipation and general sluggishness of the bowels, the administration of aloin in small doses will generally be very beneficial. Aloin being comparatively slow in its action, is not suited when we wish to empty the bowels freely and quickly; in such cases it is not to be compared with such vegetable purgatives as scammony or jalap, nor is it equal to elaterium or gamboge in dropsies; but it is far superior to any of these substances in chronic constipation with sluggishness of the liver and other abdominal viscera. In such cases I generally give it in doses of $\frac{1}{8}$ to $\frac{1}{2}$ grain, in pill, combined with iron, and in all such cases I prescribe the dried sulphate of iron, which is the best of all the ferrous sulphates for forming pills. I frequently give quinine in the same pill, combined with extract of nux vomica, and sometimes capsicum or myrrh. The following pill I have found very valuable:—

℞ Aloin., gr. $\frac{1}{4}$.
Ferri sulph. exsic., gr. iss.
Quin. sulph., gr. i.
Capsici, gr. $\frac{1}{4}$.
Extract. nucis vomicæ, gr. ss.
„ Gentianæ, q. s. ut fiat pil.
Sig.—One pill two or three times a day.

The various constituents can be varied according to circumstances.

I would only add further, that there are few indications against the administration of aloin. I have given it in pregnancy and hæmorrhoids, and have never seen any bad effects follow. When my own observations are viewed in connexion with the researches of numerous experimenters in regard to the activity of aloin, I am forced to the conclusion that aloin is the only active principle contained in aloes, and is sufficient to account for all the purgative properties of that medicine.

That being the case, I cannot conclude without again expressing a regret (as I did in 1875) that in the "Addendum to the British Pharmacopœia of 1867," published in 1874, no mention is made of aloin. It ought to find a place amongst the medicines recognised in the British Pharmacopœia. It possesses the following advantages over the crude drug:—

1. Being uniform in strength, its dose can be accurately determined.
2. Its dose being ⅛ to one grain (usually ¼ grain), it can easily be introduced into tonic pills without making these pills too large.
3. By using the active principle, we get rid of all impurities, which are so apt to cause griping.

Its uniformity in strength, the smallness of its dose, and the certainty of its action, should commend it to the favourable consideration of all medical practitioners.

# Part Second.

## REVIEWS.

*Traité de la Diphthérie.* Par. A. SANNÉ, Docteur en Médecine, Ancien Interne des Hôpitaux de Paris. Pp. 634. Paris: 1877.

(*Concluded from page* 830.)

THE portion of Dr Sanné's work which treats of the *etiology* of diphtheria begins with a condensed history of the principal epidemics which have prevailed in France and other countries since 1862. The author does not go back to earlier times, considering it unnecessary to reproduce well-known accounts of epidemics prior to that date. He refers to them, however, for purposes of explanation and comparison.

"In certain great towns—notably in Paris—diphtheria," says the author, "has become endemic. Since 1856 it has reigned continuously in that capital, with frequent exacerbations; as has been shown by discussions in the scientific societies, and by the reports of the Commission appointed to inquire into prevailing diseases. In England and in Germany its progress has been similar. In many localities, therefore, it is difficult to follow the advance of the disease and to study the conditions under which it has become developed. The necessary information on these points can only be furnished by epidemics. They enable us to investigate the influences of climate which preside over the hatching of diphtheria, and to inquire into its mode of transmission."

In the passage now quoted, we have an announcement of high importance, to the effect, that diphtheria has in later years become endemic in certain great towns—notably in Paris—and that in England and in Germany there has also been an increase in the endemicity of the disease. Whence this comparatively recent installation of diphtheria in Paris and other large towns, which, in past times, it only visited at long intervals? For a long period diphtheria, in France, was confined to certain central departments, and to some widely-scattered and limited localities. Now, there are few districts of France in which it has not become both endemically and epidemically a familiar scourge. What has happened, and is now happening in France, has nearly contemporaneously occurred, to a greater or less extent, in other countries. The fact is indisputable. How is it to be explained? This question is answered, in some degree, by the following passage from Dr Sanné's work :—

"Malignant diphtheria," says the author, "has only been observed in Paris during a limited number of later years—that is to say, since the epidemics of 1842 and 1843, described by Boudet and Becquerel. It then disappeared during a long interval, and did not again show itself till 1855. After that date it returned in epidemic outbreaks; and it finally installed itself as an endemic. The epidemic of 1855, the signal for its definitively taking possession, was coincident with the great development of means of communication throughout the whole of Europe, and particularly in France. Paris has now become to a great extent the *rendez-vous* of the entire world.

"It is not surprising that epidemics formerly limited to certain parts of France and of foreign countries should have been imported into the capital. Cholera supplies a similar example. Its latest outbreaks became generalized all over Europe with a rapidity unknown in respect to those which preceded the extension of the means of rapid intercommunication. This statement is placed beyond the possibility of doubt by a comparison of the cholera epidemics of 1832 and 1849 with those of 1855 and 1866. Everything goes to prove that diphtheria has been similarly influenced. From many parts of France, concurrent information, supplied by resident medical practitioners, establishes a coincidence between the arrival of diphtheria and the opening of railways in their respective localities. Till the railways were opened the local practitioners knew the disease only by name. Diffusion of the morbid germs by the creation of rapid means of travelling, and the active circulation of people therein originating would, therefore, seem to be the causes of the extension of diphtheria."—(P. 322.)

Railways, and the consequent enormously increased extent of travelling and the more intimate relations established by them between centres such as Paris and all other places, furnish probably the explanation of the much more rapid spread of epidemics of diphtheria in Paris since 1855; but they do not explain why diphtheria is now permanently installed in certain large towns, and notably in Paris. The explanation of that fact must be looked for in conditions pertaining to these places themselves, and not to increased intercommunication. In Paris it is not difficult to point to architectural peculiarities, and social arrangements contingent on these peculiarities, which perpetuate foci of infection, and disseminate contagious diseases. In the rebuilding of Paris sanitary requirements have been neglected. The spacious avenues and adjoining groups of beautiful streets—the tree-adorned, cleanly swept pavements—the carefully watered, dustless roadways, and the stately houses—naturally command the surprise and admiration of the stranger. His first impression is that in the gigantic enterprise of demolition and rebuilding, the dictator-edile of the Second Empire not only secured military advantages to the Government, created magnificent thoroughfares, and produced admirable architectural effects, but likewise promoted in a high degree the sanitary interests of the population. The impression is natural; but it is an impression based on superficial observation, and much at variance with facts. The building plan, which has been followed with wearisome sameness, prevents the possibility of cross-ventilation of the apartments piled up one above another in six or seven stories. The direct access of air and the admission of direct sun-light are seldom possible except by the front windows. All the windows to the sides and back are hemmed in by lofty house-walls. These walls rise everywhere in close proximity. Sometimes they form narrow uncleanly shafts, and sometimes shut-in courts, resembling large deep wells admitting light only from the top,—a contrivance which allows the houses to be squeezed together back to back to any depth from the street, so utilizing for house-room and stabling the maximum of every proprietor's area. Horses and human beings are lodged in the closest proximity. It is obvious that this reckless arrangement of habitations must not only seriously sap the health of the population, but must likewise prevent the eradication of contagious germs when once they have gained admission into a block of buildings, whether it be by a leak in the sewer or by the importation of persons affected with contagious diseases. The impossibility of purifying the dwellings by cross-ventilation makes them—when once contaminated—perpetual foci of infection.

In addition to the want of cross-ventilation and the insufficiency of direct sun-light, there exist as a rule—to which the exceptions are very few—two other flagrant defects in the stately five, six, or seven-storied houses which embrace the dwellings of the large and wealthy population of New Paris—the absence of lodgings in each "apartment"[1] for each inhabiting family, and the aggregation of all the domestics belonging to all the families in a rookery, generally filthy, at the top of the house, where usually also rooms are occupied by persons or families of respectability or without respectability, as the case may be, but generally by a community belonging to the penultimate social stratum. The cœnibicity of the male and female servants of all the families crowded in the large buildings now referred to, their isolation at night from their employers, their association with, or at least enforced contiguity to needy persons of a low class, naturally leads to immorality and disease—naturally leads to the installation among the rich and comfortable classes of an irrepressible agency for the dissemination of contagion. The compulsory social arrangement now briefly described is the plague-spot of Paris—the producer of innumerable disseminating hotbeds of moral and physical pollution. It is a thousand times more demoralizing than ever was the reprobated "bothy system" of agricultural Scotland.

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[1] In the phraseology of France, an "apartment" does not, as with us, mean a single room, but a suite of rooms. The "flat" or the part of a flat constituting the dwelling of a family, is called "an apartment" in Paris.

Another noteworthy defect in Parisian domestic architecture is the absence of sash-windows admitting of easily-regulated horizontal openings at the top sufficient for ventilation, and yet not so large as to reduce the temperature of the room in cold weather, or subject those who inhabit it to dangerous draughts. Casement windows are only suitable for day-time in summer weather. If we open the entire window, or the greater part of it, we nearly equalize the temperature of the room with that of the external air, which at night, or in winter, is not desirable; and if, adopting the other alternative, we open the window to a small extent, we are pretty certain to establish a mischievous draught between the perpendicular chink and some of the several doors with which even a very small Parisian room is provided.

Paris, as a beacon, points to the disastrous results of ignoring the necessary cross-ventilation of dwellings, and of otherwise infringing the natural laws of health in the construction of houses. Evils very similar in kind, though not so serious in degree, exist, and are now increasing in other large towns, where the costliness of space and the crowding of people are dangerously advancing hand in hand. It is impossible to prevent contagious diseases being imported into Paris and other large towns, and extending under favouring influences of season, at least to some extent, as epidemics; but it is possible to prevent their installation as endemic scourges, and to reduce their epidemic spread to an insignificant minimum.

The greatest foe of cholera, fever, diphtheria, and every kind of pestilence, is a strong gale of wind; but no purifying, searching sweep of wind can pass through dwellings surrounded, like those of New Paris, by lofty walls on at least three of their four sides, and generally on all sides, in those massive blocks extending back from the gay street frontage. The modifying and exterminating influences of violent winds on the fevers of the West Indies is well known. Hurricanes often convert the most malignant types of yellow fever into manageable intermittents, and quickly cleanse the most infected districts from endemic diseases, leaving them healthy for years.

In the Old Town of Edinburgh, less than forty years ago, when Edinburgh was not the great centre of railways which it now is, typhus was endemic; and it as well as other fevers were then epidemic at short intervals. These fevers seldom showed themselves, and never took root in the New Town, which, from its open straight streets and exposed situation, was constantly being swept by the high winds for which our city is famous, but which could not sweep through the fever-dens of the Old Town—the narrow lanes ("closes") inhabited by a poor population, crowded into gigantic houses ("lands") of from ten to fourteen stories, into which air and light only found indirect and very imperfect admission. By well-directed improvements, carried out by the municipality, the fever-dens have been demolished, and wind, as well as air and light in abundance, have been freely admitted to the Old Town, with the result of a very striking diminution in the ordinary average of fever cases, and the non-occurrence of a fever epidemic for many years. We believe we express the general opinion, when we attribute the disappearance from Edinburgh of its fever scourge in a great measure to the cross-ventilation now obtained, even in the most crowded districts, by the carrying out of well-devised plans of city improvement. The improvements, no doubt, also, at once produced good, by sweeping away many of the famous old fever-nests familiar to some of our readers who worked as dispensary pupils in Edinburgh in the time of Professor Alison, thirty years ago, and in years more distant. In addition to the destruction of fever-nests and the introduction of cross-ventilation into the former haunts of fever, another measure of immense utility has been now in operation in Edinburgh for a good many years—the maintenance of a special sanitary staff, one of the chief duties of which is to remove to hospital as quickly as possible all cases of infectious disease occurring among the poor. Speedy isolation of infected persons is thus secured, and disease is "stamped out" which might otherwise have desolated a neighbourhood and disseminated itself indefinitely. A special staff charged with the same duty ought to exist in all large towns. In cities built like Paris and some parts of Edinburgh, this isolation is a necessity, from the manner in which families are piled one above another in gigantic houses, which are really upright streets—streets with one end in the ground and the other in the air. The necessity is enormously greater when, as in Paris, the upright streets are squeezed together, back to back, with the most reckless disregard of the health of the inhabitants. We are at present dealing with well-known facts, and not with theories. Since Dr Littlejohn became Health Officer in Edinburgh in 1863, he has assiduously directed his efforts to the early discovery and speedy removal to hospital of all infectious cases. To the rigorous adoption of that system, the demolition of fever-nests, and the establishment of cross-ventilation in the "upright streets" and other close situations, the extinction in Edinburgh of its old fever scourge must be attributed.

We have gone into this question at some length on account of its great importance, and because we are convinced that it admits of being satisfactorily carried beyond the point at which Dr Sanné leaves it; and beyond which, perhaps, it might have been out of place to have discussed it in his work. Assuredly, what has been done in Edinburgh may be done in any other city where there is sufficient strength of enlightened public opinion to repress the avarice of house-builders, to regulate building speculations in accordance with sanitary requirements, and to convince ratepayers that money spent in efficient supervision of the health of the community is a great municipal economy. To sum up:—Railways are constantly importing infectious diseases into towns; but their installation and spread in towns may be prevented. Did space permit, numerous illustrations might be adduced to show, that while railways bring contagious disease to our great towns, science and common sense, by taking counsel together, may prevent their permanent installation or epidemic spread.

In speaking of the *nature of diphtheria*, Dr Sanné shows that the malady is contagious. He also makes it very clear that the false-membrane is the result, and not the starting-point, of infection. The degree in which the false-membrane exists bears no relation to the degree in which the patient suffers from diphtheritic poisoning. "There is often seen," says the author, "thin and isolated patches of false-membrane, which we should be glad to regard as herpes of the pharynx, and which by many is supposed to be that affection, but which is accompanied by very severe general symptoms of diphtheria, terminating in death. On the other hand, it is not unusual to observe large false-membranes spread over the entire throat, almost unattended by any constitutional disturbance, and ending favourably, unless they occasion death by a purely mechanical process—that is to say, by penetrating the larynx. Does this statement mean that false-membranes undergo no alterations, or that any alterations which may take place in them are unattended by inconveniences? Certainly not. In severe cases they undergo a real putrefaction, by which a secondary is added to the primary poisoning. The two differ in their nature. The pus, the altered blood, and the fœtid acrid discharge which flows profusely, are absorbed by the mucous membrane of the digestive organs, or by the denuded surfaces, so that they become a new cause of poisoning—not of diphtheritic infection, but of infection by septic products, or, in other words, by putrid infection or septicemia."

*Treatment* is discussed at considerable length, and in an able, thoroughly practical manner. Dr Sanné points out the uselessness of local treatment, except when the false-membranes, by localization on important organs, interfere with their functions. In such cases the false-membranes must be regarded as foreign bodies, measures being taken to promote their expulsion. The medicines termed "alteratives" are denounced as dangerous, so-called "specifics" are declared to be useless, and the benefits derived from "reconstituants" are judiciously insisted on. The following are the concluding sentences of Dr Sanné's remarks on treatment:—"None of the means which I have mentioned can prevent the formation of false-membrane. None of them exercise an influence on diphtheria which can properly be designated *curative*. The specific for the disease has still to be discovered. Will it ever be discovered? Probably not. All existing knowledge leads to the belief that there no more exists a specific for diphtheria than for typhus, measles, and the like maladies. To persist in searching for specifics is to lead medicine into a false course. The therapeutics of diphtheria, properly understood, are not regulated by the disease, but by the indications which the patient furnishes." —(P. 415.)

*Tracheotomy* in all its bearings on diphtheria—historical, statistical, and operative—is discussed in an exhaustive and masterly manner. The author begins by stating the position which the operation now holds in the treatment of the disease, and the high merit which belongs to Trousseau for having given it that place. "When nothing," he says, "can stop the progress of the malady, and when asphyxia, already begun, threatens to become complete, the one hope which remains is recourse to tracheotomy. This valuable operation has snatched from death many patients otherwise irrevocably lost. The highest claim of Trousseau to the gratitude of posterity is his having brought this resource into common use."

Dr Sanné's statistical review of tracheotomy in diphtheritic croup is most interesting and instructive; but, from a practical point of view, it is not necessary to rest the claims of tracheotomy upon statistical results. Were a very much smaller number of patients saved by tracheotomy than is the case, the operation would still maintain its place, as the sole means of prolonging life in cases, otherwise hopeless, so as to give time, and with time, the patient's only chance of recovery. It is a curious, and not easily explained, fact, that Italian physicians seldom give their asphyxiated cases the chance of recovery which tracheotomy affords. The number of operations in Italy have been few, and the success attending them very small. Valeriani of Turin has operated in three cases, in one of which recovery took place. At Florence, where diphtheria has been a terrible scourge for more than ten years, tracheotomy is rarely resorted to, and then, generally, it would appear, when it is too late to be of use. In nine cases of tracheotomy reported by Rosati of Florence, there was only one recovery. He attributes this mortality to the too long delay in operating. The explanation is probably correct; but the one case saved is more than sufficient to justify the nine operations. Let us here remind the reader that, akin to delay, there is another frequent cause of non-success in tracheotomy with which the practitioner, and not the disease, is chargeable, viz., certain treatment adopted to give the patient a chance of recovery without tracheotomy, but which in point of fact seals his doom, and deprives him of the chance of recovery by means of the operation. We refer to the strength subtracted by the agency of antimonial emetics, and by the struggles and violent resistance excited by the forcible application of caustics, solvents, and other substances to the interior of the throat. We have not space to enter into the statistical questions connected with the operation. We would just remark, that the cases are not sufficiently similar to admit of strict comparison either as single cases or as groups of cases. The subjects of some operators have been chiefly robust and wholesome, the country-bred children of parents in comfortable circumstances; while the subjects of other operators have been chiefly the scrofulous, etiolated, puny progeny of the degenerate proletarian population of a large town. Some statistics are derived wholly, principally, or partly from hospital practice, or from private practice, as the case may be. One practitioner avoids operating in unpromising cases; another will always operate if he thinks the operation affords the slightest hope in a case which must otherwise inevitably perish. Some operate too soon, and others too late. Again, the constitution of the prevailing epidemic greatly affects statistical results. The mortality in the Hôpital des Enfants Malades of Paris after tracheotomy in diphtheria has been much above the average during the last three months, owing to the unusual malignity of the cases.

As might be expected, more recoveries follow the tracheotomies performed in family homes than those performed in hospitals. In the former class of cases, the subjects are better, and the minute details of after-treatment are more easily carried out; and then again, many of the cases operated on in hospitals have been badly attended to before admission, and when admitted have been almost at the last gasp. Dr Sanné gives a table exhibiting the tracheotomy statistics of diphtheria in the Hôpital S. Eugénie of Paris for twenty-two years, viz., from 1854 to 1875 inclusive. During that period the number of patients operated on was 2312, of which 509 recovered, being 1 in 4·54. He also gives a table exhibiting the tracheotomy statistics of diphtheria in the Hôpital des Enfants Malades of Paris for twenty-five years, viz., from 1851 to 1875 inclusive. During the twenty-five years, 2351 patients were operated on, of whom 614 recovered, being 1 in 3·82. The average recoveries, taking together all the hospitals of France, is 30 per cent. In the second edition of his Clinical Lectures, published in 1864, Trousseau announced that more than one-fourth recovered of 200 patients on whom he had operated. The greatest recorded success, we believe, is that of Dr Révilliot, brought by him before the Société Médicale des Hôpitaux on the 12th May 1866. At that date, probably, Dr Sanné's statistics were completed in print, as he does not mention Dr Révilliot's cases. Dr Révilliot was a pupil of Dr Barthez at the Hôpital S. Eugénie before he settled in Switzerland. From 1868 to the date at which he made his communication to the Société Médicale des Hôpitaux, he had operated on 87 patients, 16 of whom were under two years of age. Of the 87 operated on, 33 recovered; that is, 43·6 per cent. This high percentage of recoveries, altogether extraordinary, was probably the result of several favourable conditions. Of the entire number of 87, no less than 84 were operated on in family homes, and 3 only were operated on in an hospital; and of the 87, nearly one-half, viz., 47, lived in the country. Possibly the proportion of malignant cases was small, or there may have been none. The operator may have been expert, and his after-treatment may have been unusually good. The degree of success which Dr Révilliot met with is exceptional. The circumstances under which it occurred convey instruction. Without pursuing the subject further, we would remark that enough has been said to show that general statistical results do not affect the question of operating in any individual case. A rule based on numbers would be absurd. No such rule or rules can be formulated. The only point to be considered is—Does tracheotomy afford an otherwise non-existing chance of life? Asphyxia is the indication. There is some difference of opinion as to the degree to which this state ought to be allowed to proceed before we operate. No more precise rule can be laid down than this—operate whenever the asphyxia threatens life. In each individual case the decision must, therefore, be special and the result of the physician's experience and sagacity.

The instruments required, or which have been recommended on authority, are interestingly described, and are commented upon in an instructive manner. The preparations for the operation, the accessory arrangements, and the nature of the assistance which it is desirable for the operator to possess, are all fully explained. The advantages and disadvantages of the different methods of operating are clearly expounded. Three principal methods and their variations are described in detail, viz., inferior and superior tracheotomy, and crico-tracheotomy. In *inferior* tracheotomy—the operation recommended by Trousseau—the trachea is entered below the thyroid body, often in the space between the third and seventh rings. In *superior* tracheotomy, the incision is made from the lower edge of the cricoid cartilage, in the space which comprises the upper two or three rings. In crico-tracheotomy the incision is made in the cricoid cartilage and the first two upper rings. Dr Sanné regards "inferior" tracheotomy as difficult and dangerous, and he recommends "superior" tracheotomy as preferable to it and to "crico-tracheotomy." Besides controversy as to the situation of the incision, there has been much discussion regarding the propriety of operating slowly or rapidly. The author adopts a method which is a modification of the rapid proceeding—to use his own words, "*un procédé mixte qui se rapproche de la methode rapide.*" He, however, indorses Trousseau's recommendation of the slow method [*opération lente*] to the numerous practitioners who are not surgeons by profession, and who are seldom called on in their ordinary practice to perform tracheotomy. The advantages of rapidity of execution are insisted upon. "The rapid method," says the author, "is much the least fatiguing; it much less exposes the patient to syncope and death during the operation; it ought to be specially preferred when the patient is in an advanced state of asphyxia. It involves, no doubt, a greater risk of hæmorrhage; but then it offers an immediate remedy for this inconvenience, for the introduction of the canula is the best hæmostatic, rapidity of execution extinguishes the evil as soon as it arises. But that matters may so proceed, the operator must be expert in tracheotomy. If hæmorrhage occur, and he be slow to find the trachea, slow to incise it, and slow to introduce the canula, all will be lost: the protracted nature of the operation will cause the patient to succumb under the influence of asphyxia or hæmorrhage." For complete details regarding the different operative proceedings, and for full information on every branch of the subject of tracheotomy in diphtheritic croup, we refer inquirers to Dr Sanné's work. The amount of well-arranged and clearly-expressed knowledge brought together in moderate space in relation to tracheotomy is marvellous.

In conclusion, we heartily commend Dr Sanné's Treatise on Diphtheria to the careful study of our readers. It is one of the most valuable recent additions to medical literature.

## *Dictionnaire Annuel des Progrès des Sciences et Institutions Médicales, Suite et Complément de tous les Dictionnaires.* Par P. M. GARNIER. Douzième année, 1876. Pp. 531. Paris: 1877.

GARNIER'S Annual Medical Dictionary is a compact and useful book of reference, on a different plan from any of our own similar compilations. The articles are arranged in alphabetical order; and the work embraces the principal subjects of the year relating to the medical sciences and the medical profession, including obituaries, education, and judicial decisions respecting fees and other matters.

If we wish to know what the volume contains about any particular disease or mode of treatment—say, for example, rupture of the perineum, fistula, or smallpox, we turn respectively to "*rupture* *de périnée*," "*fistule*," and "*petite variole*." Under the first, we find an abstract of a paper by Dr Matthews Duncan, which appeared in our number for April 1876; under the second, we find abstracts of papers by different authors on different kinds of fistula, including that by Mr Joseph Bell on Vesico-vaginal Fistula, which was published in February 1876; and, under the third, we are furnished with varied information regarding smallpox. The gleanings of the year bearing upon the new doctrine of the evolution of smallpox epidemics are brought under notice, viz., the doctrine that apparently successive epidemics are really renewals of the same epidemic seemingly extinguished, but only exceedingly reduced for the time being, by the operation of temporary causes unfavourable to the spread of the variolous contagium. The traffic in rags and old clothes is shown to be an active agency in the diffusion of smallpox, by reference to facts mentioned by Dr Gilbert in the *Marseille Médicale* for February 1876.

If we wish for information regarding the important discussions and decisions of the French Parliament in 1876, in respect to the reorganization of the system of medical education, we turn to the word "ENSEIGNEMENT," under which we find the results of the debates in the Chamber of Deputies and in the Senate, together with an account of the spirit and tendencies of the speeches of the clerical party and of their vigilant opponents, the friends of scientific progress and unfettered teaching. We find that stormy debates have ventilated truth, and are leading to substantial reforms likely to be progressive. The salaries of the professors have been augmented, and new professorships have been founded. An amendment proposed by Dr Clémenceau, and adopted by the Chamber of Deputies, has created a chair for Clinical Instruction in Mental diseases in the Medical School of Paris. It is worthy of notice that the establishment of this new clinical chair took place simultaneously with permission being given to Dr Magnan to resume his clinical lectures on insanity at St Anne's, after a temporary suppression by a despotic and ill-judged edict of the Prefect of the Seine.

We find among many interesting items regarding the medical schools of France the following paragraph:—

"FACULTY OF PARIS.—By a reliable census of the medical students of Paris, taken on the 10th February 1876, it appears that their number was 6421, of whom 300 only were military medical students. It is evident, therefore, that the reputation of the school has not declined; and that very few foreign faculties, including even those of Germany, can point to a similar afflux of pupils."

In the Dictionary for 1877, we may expect an account of the demolitions and new buildings now progressing in the Rue de l'Ecole de Médecine. The very inadequate accommodation of the school is being greatly enlarged, and its architectural effects are being beautified. In future the principal entrance will be in the new Boulevard—the Boulevard St Germain. This and the other associated changes now being carried out have necessitated the demolition of numerous houses, old and new, in the Rue de l'Ecole de Médecine, including book-shops and several lodging-houses frequented for the last forty years, at least, by Parisian medical students, French and foreign.

If we wish to know how death has dealt with our profession in 1876, we turn to the word "NÉCROLOGIE," under which we find obituary notices of thirty-one medical celebrities, and of an *interne* who died of diphtheria contracted at the *Hôpital des Enfants Malades.* Among the most noteworthy names in this Obituary Record is that of ANDRAL (Gabriel) "*—la plus grande figure du Panthéon médical Français de* 1830, *qui illustra tant la France.*" Andral died on the 13th February 1876, aged 78. Of our own James Warburton Begbie there is a notice, from which the following is an extract:—"BEGBIE (James Warburton), the leading physician of Scotland, the most consulted by his colleagues and by the public on account of his kindness and skill, died of hypertrophy of the heart, aged only fifty years, on the 25th February 1876. Without official or university title, the author of no special or remarkable works, he conquered for himself the first place at Edinburgh by his personal qualities. He received annually, in fees, from four to five hundred thousand francs (£16,000 or £20,000). His activity was equalled by the attention which he bestowed upon his patients poor and rich, and by his enlightened diagnosis and treatment. These were the sources of his extraordinary success and popularity. His kindliness of manner rendered him peculiarly agreeable and fascinating."

It would be easy, but not quite fair, to find fault with the compiler for omissions. The field from which selection had to be made was so wide, that diversity of opinion will exist as to the facts which most deserved to be recorded, and the relative space to which each topic was entitled. So far as we can discover, the inaccuracies are few, and comparatively unimportant. Taking it all in all, Garnier's Dictionary for 1876 is the successful result of much industry and conscientious research.

*A Systematic Handbook of Volumetric Analysis, or the Quantitative Estimation of Chemical Substances by Measure, applied to Liquids, Solids, and Gases.* By FRANCIS SUTTON, F.C.S., Public Analyst for the County of Norfolk, etc. etc. Third Edition. London: J. & A. CHURCHILL. 1876.

THIS "Handbook" is undoubtedly the standard work on Volumetric Analysis in our language. The various processes are clearly described, and this *third* edition embodies all such novelties and modifications as experiment has proved to be worthy of notice. Mr Sutton, in the first part of his work, defines clearly the distinction between *gravimetric* analysis or quantitative analysis by *weight*, and volumetric analysis or quantitative analysis by volume. He says: "The leading principle of the method consists in submitting the substance to be estimated to certain characteristic reactions, employing for such reactions solutions of known strength, and from the volume of solution necessary for the production of such reaction, determining the weight of the substance to be estimated by the aid of the known laws of chemical equivalence. Volumetric analysis, or quantitative chemical analysis by measure, in the case of liquids and solids, consequently depends upon the following conditions for its successful practice:—

1st, A solution of the reagent or test, the chemical power of which is accurately known, called the "standard solution."

2d, A graduated vessel from which portions of it may be accurately delivered, called the burette.

3d, The decomposition, which the test solution produces with any given substance, must be of such a character that its termination is unmistakable to the eye, and thereby the quantity of the substance with which it has combined accurately, determined.

Mr Sutton next gives an example illustrating the above general principles, which we quote, as showing the clear and precise way in which he explains the various processes:—"Suppose, for instance, that it is desirable to know the quantity of pure silver contained in a shilling. The coin is first dissolved in nitric acid, by which means a bluish solution, containing silver, copper, and probably other metals, is obtained. It is a known fact, that chlorine combines with silver in the presence of other metals, to form chloride of silver, which is insoluble in nitric acid. The proportions in which the combination takes place are 35·5 of chlorine to every 108 of silver; consequently, if a standard solution of pure chloride of sodium is prepared by dissolving in water such a weight of the salt as will be equivalent to 35·5 grains of chlorine ($= 58{\cdot}5$ grains NaCl), and diluting to the measure of 1000 grains, every single grain measure of this solution will combine with 0·108 grain of pure silver to form chloride of silver, which is precipitated to the bottom of the vessel in which the mixture is made. In the process of adding the salt solution to the silver, drop by drop, a point is at least reached when the precipitate ceases to form. Here the process must stop. On looking carefully at the graduated vessel from which the standard solution has been used, the operator sees at once the number of grain measures which have been necessary to produce the complete decomposition. For example, suppose the quantity used was 520 grain measures; all that is necessary to be done is to multiply 520 by the co-efficient for each grain measure, viz., 0·108, which shows the amount of pure silver present to be 56·16 grains." Mr Sutton next gives a very good description of the various instruments and apparatus necessary. This part of the work is illustrated by an extensive series of very admirable drawings. Part II. is occupied with a description of Analysis by Saturation. Part III. Analysis by Oxidation or Reduction; and Part IV. Analysis by Precipitation. Part V. is occupied with the Application of the foregoing Principles of Analysis to Special Substances. Part VI. is a most important chapter. It contains Analyses of Urine, of Soils, of Manures, of Natural Waters and Sewage, and other Substances. Part VII. is chiefly occupied with the Analysis of Gases.

The work is one which we have much pleasure in highly recommending to the notice of all who take an interest in chemical analysis. No better book on this subject exists in the English language.

*The Journal of Anatomy and Physiology.* Conducted by G. M. Humphry, M.D., F.R.S.; Wm. Turner, M.B.; Michael Foster, M.D., F.R.S.; and Wm. Rutherford, M.D., F.R.S. Vol. xi. Part 1.: October 1876: Macmillan & Co.: Cambridge and London.

This number of the *Journal of Anatomy and Physiology* contains some excellent material. In addition to the Report on the Progress of Physiology by Dr Stirling, there are thirteen original articles, the majority of which are contributed by authors well known in the scientific world.

The first paper, upon "The Development of the Mamma and of the Mammary Function," by Dr Creighton, is well written, and bears evidence of close research. He strongly objects to the generally accepted view that the mamma is developed by a process of budding from the mucous layer of the skin, and challenges the accuracy of the homology which is believed to exist between this organ and sebaceous glands. He affirms that in the guinea-pig the process of development of the mammary acini is step for step the same as that of the fat-lobules; that they develop at many separate points in a matrix tissue, and that the cells from which they spring are identical with those from which the adipose tissue takes origin. Further, he urges that in point of time the ducts develop first, and that they arise from the same matrix tissue of the aggregation of the embryonic cells in linear series.

Succeeding this article is one by Professor Turner, "On the Placenta with Especial Reference to the Theory of Evolution." This is a paper of very great interest and importance, and in this respect, therefore, it is quite in keeping with the elaborate memoirs by the same author and upon the same subject which have preceded it. Some of the conclusions arrived at by the author may be summarized as follows:—In mammalia the same essential constituents are found entering into the formation of all placentæ, and the simplest arrangement of these may be looked upon as constituting a placenta in its most generalized form. Such a fundamental type of placenta would consist of a fœtal portion, composed of a vascular membrane, upon the one face of which is laid down a layer of pavement epithelium, and a maternal portion similar in constitution, with the exception that in this case the epithelium is of the columnar type. These two membranes are applied to each other, so that their epithelial surfaces are in contact. From this simple type-form the different varieties of placenta may be conceived to be evolved, the process of evolution being effected by "the assumption of a greater extent of complexity in the foldings, on the one hand of the villous chorion, on the other hand of the uterine mucous membrane, with, in addition, in some placentæ, modifications in the relative size of the maternal bloodvessels, and in the form of the maternal epithelial cells." The diffused placenta presents the closest affinity in structure to the fundamental type, whilst the human placenta is the most specialized. But the author goes on to explain, that whilst this evolution of the more complicated placentæ out of the more simple placentæ is quite conceivable, yet the evolution cannot be regarded as taking place as a continuous process, "from the diffused through the polycotyledonary, zonary, and dome-shaped group-forms, until at length the highly specialized discoid placenta of monkeys and of man has been produced."

The practical value of an article by Professor Rutherford and M. Vignal, giving the results of a second series of "Experiments on the Biliary Secretion of the Dog," cannot be too highly estimated. By these experiments, certain drugs at present almost unknown to the physician, are shown to possess most important cholagogue properties and in different degrees, whilst the therapeutic action of others is explained. Thus, the oleo-resin iridin, obtained from the root of *Iris versicolor*, is proved to act on the liver most powerfully, and also to stimulate the secretion of the intestinal glands, whilst its action is gentler than that of podophyllin. Ipecacuan also is a powerful stimulant of the hepatic secreting apparatus, and the bile seen in the evacuations after it has been administered for dysentery is not due to the relief of a supposed spasm of the bile-ducts.

Of the other contributions, we might mention an elaborate paper upon "The Structure of the Retina," by Dr Thin and Mr Ewart, some supplementary remarks "On the Physiological Effects of Severe and Protracted Muscular Exercise," by Austin Flint, jun., M.D., and an account of "The Action of Pilocarpin on the Submaxillary Gland," by Mr Langley of Cambridge, as being of a character likely to excite general interest.

*The Anatomy of the Head.* By THOMAS DWIGHT, M.D., Professor of Anatomy at the Medical School of Maine, etc., etc. Boston: H. O. Houghton & Co. Cambridge: The Riverside Press: 1876.

So few anatomical books have emanated from America that we cannot avoid regarding this work, by Dr Dwight, upon the Anatomy of the Head, as being something of a novelty—but as such it should be all the more welcome.

In the preface the author points out that the object which he has in view is to give an idea of the head as a whole, avoiding as much as possible the minutiæ and details of descriptive anatomy. The book, therefore, is to be regarded as one in which the more important anatomical facts are gathered together, and placed before the reader in such a form as to show the true relationship which exists between individual parts; and we must congratulate Dr Dwight upon the success that has attended his efforts in this direction. Of course, in the writing of such a book a considerable preliminary knowledge of anatomical detail on the part of the reader must be assumed, but any one possessing this cannot fail on careful perusal of the work in question to acquire a very just conception of the relative anatomy of the head. The style in which it is written is tolerably clear, and the matter is undoubtedly good, and bears evidence not only of close personal observation on the part of the writer, but also of very extensive acquaintance with all the latest advances in anatomical science. Thus we find in his description of the skull, that he is quite familiar with the fact, first pointed out by Hyrtl, that there are in reality two curved ridges traversing the parieto-temporal region. Now this is a point which is noticed in few of our own anatomical text-books. Indeed, we believe that it was Dr Cleland of Galway who first, in this country, in a paper read before the last meeting of the British Association, called attention to it. Dr Dwight is of opinion that the lower ridge marks the origin of the temporal muscle, and he agrees with V. Jhering that the upper one serves for the attachment of the superficial temporal fascia. Again, in the chapter which he devotes to the Intracranial Circulation, he shows that he is well acquainted with Duret's recent and important investigations in this direction—the most striking characteristic of which is, that in the cerebral substance there is little or no anastomosis between the vessels of supply. In short, throughout the whole book we are constantly met with judicious selections from all trustworthy and recent memoirs bearing on the subject.

A severe critic might perhaps take exception to a certain want of precision which occasionally shows itself in the course of certain of the anatomical descriptions. Thus we find (p. 29) it stated, that the posterior pyramids of the medulla oblongata lie upon the floor of the fourth ventricle. Now, in this country, at least, anatomists are in the habit of applying the term "floor" merely to that part of the medulla which lies above the divergence of the posterior pyramids. Then in another page (p. 41) we find the following statement:—"Under the inner fold is the cavernous sinus, which actually contains the carotid artery and several nerves." From this we might be led to infer that the structures mentioned actually lie in the interior of the sinus, bathed by its blood, whereas we know, that although they are placed within the space formed by the separation of the two layers of dura-mater, they are excluded from the blood-channel by the delicate lining membrane of the sinus. And we might bring forward other instances of a deficiency of the precise expression, so essential in anatomical composition. Upon the whole, however, the style is clear and exact.

It is much to be regretted that the author has appended to his otherwise excellent book six lithographic plates representing sections of the frozen head, which are altogether unworthy of the text, whether we regard them from an artistic or from an anatomical point of view; and perhaps this is not very much to be wondered at, when the author allows that they all represent sections "from one head, that of an elderly man," and that it "was not very fresh." But in addition to this, the sections, in some cases, are made obliquely, and not at right angles to the middle line, so that two views are given, whether intentionally or not we cannot say. The natural result of this is confusion in the mind of the student. In making this criticism, we are well aware that perfect symmetry of the body does not exist; but the dissimilarity of the objects to be seen when we compare the two sides of the sections is so great that we are forced to conclude that obliquity of section is the cause.

The shortcomings of the book, however, are diminutive in comparison with its merits, and we trust that it will meet in this country with the favour that it deserves.

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*A Directory for the Dissection of the Human Body.* By JOHN CLELAND, M.D., F.R.S., Professor of Anatomy and Physiology in Queen's College, Galway. Smith, Elder, & Co. London: 1876.

WE are glad to announce the publication of this little book by the distinguished Professor of Anatomy in the Queen's College, Galway, as it promises to be a useful one to the students attending the Edinburgh School of Anatomy.

As its title indicates, it is simply a book of directions as to how, in the course of dissection, the various structures of the body may be best exposed and studied, and its value lies in the fact that it is the only book in which the order of dissection employed in this school is recommended. It cannot, therefore, in any sense be regarded as a text-book of anatomy, or a book which will supplant those at present in use; but taken in conjunction with these it will be very helpful to the student, and render him much less dependent upon the demonstrator.

The opening chapter upon the use of instruments is a most instructive one. Too great stress cannot be laid upon this point, because it must be remembered that it is in the dissecting-rooms that a student acquires in the first instance the manipulative power which is so essential to the surgeon.

Dealing with the subject under the restrictions mentioned, Dr Cleland has nevertheless made the book a very readable one; and the method and arrangement exhibited throughout cannot be too highly commended.

We trust soon to see it in general use amongst the students of this school.

*Deaf not Dumb.* By B. St J. Ackers, Barrister-at-Law, Esquire. London: Longmans, Green, Reader, and Dyer: 1876.

Mr Ackers in this lecture, now published in the form of a pamphlet of thirty pages or so, clearly shows that the term deaf and dumb, however natural it may seem, owing to the familiarity of the expression, is erroneous as applied to the vast majority of persons without hearing.

After discoursing upon the causes and prevention of deafness, and a historical sketch of the education of the deaf, in a clear and what must have been easily-understood method (by a popular audience), our author impresses upon his hearers the necessity of educating all deaf persons in what is known as the German system, *i.e.*, that which is based on articulation and lip-reading. No doubt can be entertained of the necessity there exists in this country for the "German system" in the education of the deaf, and still less can the doubt be of the advantages to be derived by the deaf and the community in general. Mr Acker brings this out clearly and convincingly in his lecture delivered before the Gloucester Literary and Scientific Institution, of which he occupies deservedly the position of President. What we require at present is a training college for the instruction of teachers in the "German system." In the meantime Mr Acker tells us what we ought to do in forwarding the good work. "All can help in some way. All can help by speaking of the deaf no longer as deaf and dumb, but as deaf *not* dumb. Once more, I beseech you, bear always in mind that there is no such thing as a person born deaf and dumb, because deaf. Let us never rest, therefore, till we have secured that in our own land, at least, the precious gift of speech be preserved, trained, and made the utmost of. Let us never rest until we have wiped out the reproach that, by our very education itself, we have made many, very many, deaf and dumb, who might and ought to have been deaf *not* dumb." The above appeal we consider worthy the notice of the medical profession, and of all who, like them, love their fellow-men, and we commend the perusal of the lecture in its present extended pamphlet form to all who desire information, relating to the education of the deaf.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION LVI.—MEETING V.

*Wednesday, 7th February* 1877.—Dr J. D. GILLESPIE, *President, in the Chair.*

I. *Dr Bruce* narrated a CASE where he had been suddenly called to see a little boy aged five, who was supposed to be in a fit. He found him lying on his mother's knee *in articulo* from croup, so that there was no time to send for a surgeon. He therefore put the boy on a table and performed tracheotomy with his penknife, using a female catheter as a canula. He then carried on artificial respiration, but without avail. As the boy was evidently dead, he opened the trachea further, and found the false membrane he now showed them lying loose in it. The case had evidently been neglected, and was certainly one where an emetic administered in time might have done much good. It threw light on the diagnosis between diphtheria and croup. The former began in the fauces, or higher up, and implicated the trachea by extension downwards; the latter originated in the trachea, and did not show any false membrane in the fauces, as in this instance.

*The President* remarked that the case was evidently one of pure croup, a somewhat rare occurrence.

II. *Dr Craig* then read a paper on THE THERAPEUTICS OF ALOIN, which appears in this number of the Journal, p. 911.

*The President* said the Society was indebted to Dr Craig for his paper on aloin, the active principle of aloes. He thought that he had made out his case, and shown that the active principle was a more refined drug than the crude one which required a larger dose. Until the paper was read he had always believed that medical men had made up their minds that aloin was the active principle of aloes, and was not aware that some did not believe in it.

*Dr Keiller* asked Dr Craig if he had used it for children, where concealment of drugging was so necessary. If he was not mistaken, Dr G. W. Balfour had shown him lozenges containing aloin which he used. They were almost tasteless, and made by Robertson, chemist.

*The President* asked if aloin was not very bitter; for, if so, as he supposed, it would be difficult to induce children to take the lozenges.

*Dr Craig* said it was so, but the taste could easily be covered.

III. *Dr Batty Tuke* then read a paper on THE DIAGNOSIS OF GENERAL PARALYSIS OF THE INSANE, which appears at page 865.

*The President* was sure the Society was indebted to Dr Tuke for his interesting communication on such an important subject.

*Dr Argyll Robertson* had listened with pleasure and interest to Dr Tuke's paper. Even from his own limited experience of mental diseases, he could corroborate what had been said as to the liability to error in the diagnosis of this disease. He had known several cases where the subsequent history of recovery showed that there had been a fault in diagnosis. It was therefore important to have some definite and distinct means of diagnosis by which they could separate more favourable diseases from this one. The cases most likely to be confounded with it were those of alcoholic poisoning. In his own department he had occasion to examine cases where there was dimness of vision owing to poisoning of the nervous system with alcohol. In general paralysis, alterations in the size, form, and mobility of pupils were not rare; but he had never seen the pupil contracted and immobile in alcoholic poisoning. Thus some stress should be laid on the ophthalmic appearances. In alcoholic poisoning they might have hyperæmia of the optic disc and retina as in general paralysis; but, as Dr Tuke had pointed out, this passed off when stimulants were withdrawn. As to the test of colour-blindness in a certain part of the field of vision, he had only recently tried it in four cases. In all but one it succeeded. In this one instance he could get no distinct signs, but this might be due to patient's inability to give a proper account of what he saw. The cases were too few to enable him to say much; but he hoped they would soon have some means of distinguishing certain defects of sight due to alcoholic poisoning from those where the impairment was owing to some organic cause. The two cases of Dr Tuke were well worthy of record on their own account, as being instances of symptoms misleading two men thoroughly well qualified to decide on them. Dr Tuke deserved commendation for giving an account of his mistake, and it would be well if every physician and surgeon in like manner pointed out his own error.

*Mr Bell* had seen, some years ago, two cases of exceedingly advanced syphilis with alcoholismus chronicus in one of them.

He had the advantage of consultation with eminent authorities, psychological and medical, and in both cases the psychologists gave a grave prognosis as to general paralysis, and in one of them it was said to be very far advanced. Under antisyphilitic remedies, however, they progressed favourably, although one relapsed in four years. In both, the retinæ were hyperæmic. He wished to ask Dr Tuke whether syphilis, apart from alcohol, ever complicated the diagnosis?

*Dr Keiller* referred to a case in point which Drs Tuke and Argyll Robertson had seen along with him. As far as his judgment went, no mistake in diagnosis was made in this instance, although there were symptoms in this case which were not strictly in accordance with the idea of mere general paralysis. The patient was a handsome well-built man, who, several years ago, got a very marked dose of syphilis. He had at that time a chancre which nearly ate off the whole of his glans penis. His health broke down under syphilitic symptoms, and, two years ago, Dr A. Robertson was consulted about his eyesight, which had failed. His mind was affected: he imagined himself a much wealthier man than he really was. Previous to his illness he had been engaged in building speculations. Mental disease from syphilis was suspected, and he was accordingly examined by Drs Robertson and Tuke, who diagnosed general paralysis. Since that time he had got worse. He had seen him lately, and found his sight and hearing much affected. Dr Keiller considered this case so unusual in several particulars, that he wished Drs Tuke and Robertson to make it a study, and he would take care to give them every opportunity of doing so.

*The President* wished to suggest that the first case narrated might be general paralysis after all, seeing that the patient had relapsed.

*Dr Batty Tuke* replied that Mr Bell, in his remarks, had anticipated the paper which he intended bringing before the Society on a future occasion. It was certainly extraordinary how insanity due to syphilis should simulate general paralysis; but the difficulty in diagnosing the former from the latter was not so great as in the case of insanity from alcohol. In regard to the President's suggestion, he thought that the existence of general paralysis in that patient was doubtful. Such a case was never heard of, as general paralysis was generally fatal in about four years. In some cases the symptoms disappeared for a few months, but always returned. Remittent general paralysis was quite incompatible with their present ideas of it. Then further, in this case they had a history of alcoholism.

IV. *Mr Annandale* then read a paper regarding a CASE IN WHICH A KNITTING-NEEDLE PENETRATED THE BRAIN THROUGH THE ORBIT. This paper appears at page 891.

*The President* expressed the thanks of the Society to Mr Annandale for his interesting case. He, himself, had seen an accident of a milder description which showed how much injury could be done with little external sign of it. The patient was a gentleman's servant, who had gone to a *battue* to load his master's spare breechloader. Suddenly the master noticed that one of the party was aiming point-blank at him. He accordingly ducked his head, and the result was that his servant was shot in the eye. When the man was first examined no pellet was found. He himself, however, probed a wound in the eyeball, and found a pellet there, which, assisted by Mr Joseph Bell, he extracted. Blindness resulted—due, as Mr Walker found, to the retina, from the violence of the shock, being curled up. The patient, with the exception of his eye, recovered all right, but lost his place as butler from the miscalculations as to distance he made in handing round dishes. He had, however, regained this power, and was now engaged in his occupation as a waiter. He wished to know if Mr Annandale could tell them how far the knitting-needle had penetrated in his case.

*Mr Bell* remarked on the interest of the case, and wished to narrate one which had recently come under his notice. Last Friday, a respectable young man left a party quite sober, but remembered nothing more until he found himself in a police-cell. He was brought to the Infirmary, where one of their staff examined him and found a bruise over the left eye. He was not, however, detained in Hospital. He, himself, saw him on Saturday morning, and at once noticed that there was something serious the matter. There was no bruise, and no welling from the ear; but the patient was almost absolutely deaf. The natural idea was fracture of the base; but there was no external mark, and no alteration of the pupils.

*Dr A. Robertson* remembered Mr Annandale's case, which happened about a year ago. The ward was so light, and the patient in such a state, that ophthalmoscopic examination could not be made, so that he could say nothing as to the state of the retina. The movements of the eyeball were perfect—a fact important in considering the nature of the lesion. From the formation of the orbit, and the position of the foramina, one would imagine that a foreign body, sliding along its roof, would pass into the sphenoidal fissure, and not through the optic foramen. Passage through the former would not cause instant blindness; but the other nerves (the third nerve, etc.) would be injured. He therefore believed the knitting-needle had perforated the optic nerve. This would be rare, because the sheath of the optic nerve was very tough, and the foramen was accurately filled up. The needle must have gone between the nerve and the edge of the foramen. The after-occurrence of atrophy of the optic nerve did not of itself necessarily indicate that the nerve had been directly injured.

*Mr Annandale* explained, in answer to the President's question, that the depth to which the needle penetrated was not known. In the experiments he had performed, through Dr Wyllie's kindness, he found that there was a tendency for the instrument to pass through the sphenoidal fissure. By varying the direction of the needle, however, it could be made to enter the optic foramen between the nerve and the foraminal edge. From the *ptosis* being only temporary, the third nerve was probably uninjured.

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## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXXVI.—MEETING V.

*24th January* 1877.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Matthews Duncan* showed a specimen illustrating FRIABILITY OF THE CORD, and a female fœtus in the third month of pregnancy with so-called TORSION OF THE CORD, for the latter of which he was indebted to Mr Maxwell. The more specimens he saw of this latter lesion, the more did he think it was, in many cases at least, an atrophy rather than torsion of the cord. When the cord lost its jelly, and the vessels therefore became prominent, it had a look of torsion; and even ordinary spirality of the vessels suggested torsion. In this case the lesion was at the navel, its most frequent position, and there was no appearance of torsion. Perhaps the structure of the cord at this part, as shown by Virchow, Lawson Tait, and others, accounted for its frequency at the navel. Dr Duncan had seen, repeatedly, specimens shown as torsion when the cord was separated and still retained the twisted appearance. He thought that, to prove torsion, it should, when separated, partially untwist itself and lose the appearance of twistedness. The degree of twisting or number of torsions might be made out by previously transfixing with a needle, and observing the untwisting. Dr M. Duncan thought his specimen of friable cord might throw some light on this matter. It was from an ill-developed male child, a few weeks premature probably, the son of healthy parents. The disease affected the fœtal end of the cord, diminishing as the distance from the navel was increased. It was a fat or plump cord. At the navel it at once attracted his attention by its cheesy hardness. He resolved to tie it carefully; but the ligatures of twine thread cut it as if it were cheese; and the same happened when he used tape. Finally, he carefully tied it with tape; but he wished he had had Dr Dickson's elastic silk tape which he recommended for tying all cords.

On examination of this cord by Dr Underhill, at his request, it was found that, on microscopic investigation of sections, hardened in Müller's fluid and frozen, parts of the cord are perfectly normal; parts are morbid. If a line were drawn bisecting it, and having the two arteries on one side and the vein on the other, the arterial half is mostly normal. On the half containing the vein, one is struck, on naked-eye inspection of the sections, by a broad band of tissue more deeply stained than the rest passing in an irregular curve round the vein at a short distance from it, and between it and the external surface of the section. This band is found to be composed almost entirely of round granular lymphoid cells, which lie in great masses, replacing, or at least hiding, the proper tissue of the part. This band extends over fully half the section; the cells are, however, not confined to it, but are scattered in smaller numbers over that half of the section, partly lying within the meshes of the trabecular structure, partly upon and among the trabecular fibres and cells; a few are found in the other half of the section where they are confined to the immediate neighbourhood of the bloodvessels. These lymphoid cells contain a large nucleus, some few of them two, which are coloured deeply by hæmatoxylon, and to a less degree by carmine. In a normal cord, which was examined for the purpose of comparison, there are none of these round cells to be found.

II. *Dr Matthews Duncan* showed in the fœtus above spoken of as dying from torsion of the cord a PECULIAR HYDRORACHIS. It was of the size of a walnut, and its peculiarity was that it was truly neither a hydrorachis nor an encephalocele, for the sac communicated with the cerebro-spinal cavity by a hole in the posterior part of the occipito-atloid articulation sufficient to admit a large probe. It filled up the space between the occiput and neck.

III. *Dr Matthews Duncan* exhibited a remarkable specimen of an ANENCEPHALOUS FŒTUS, in the fourth month of pregnancy, for which he was indebted to Dr Jamieson. There was absence of the whole anterior abdominal wall, or extreme EXOMPHALOS; the abdomen being closed by an expansion of the cord.

IV. *Dr Cuthill* then read a paper on THE ACTION AND THERAPEUTIC USE OF THE ERGOT OF RYE.

*Professor Simpson* testified that from his experience he could not doubt the powerful effect which ergot exercised on the muscular fibres of the uterus. He had no experience of its effect on other unstriped muscles, but he thought it reasonable to infer that it should act in some way on them. He had found it a remedy of much service in some affections of the unimpregnated uterus, its power of producing contraction and shrinking being most marked, especially in cases of fibrous tumour.

*Dr Allan Jamieson* mentioned the fact that in certain districts cows, having partaken of ergotized grasses, aborted, thus proving the action of ergot on the uterus. Curiously enough, in those districts in which there were many geese, who were very fond of eating the ergot, the cattle were not so readily affected in this way.

*Dr James Young* bore testimony to the powerful effects which, in his experience, ergot had on the uterus. In a patient whom Dr

Simpson saw with him, the bleeding from a fibrous tumour had been greatly controlled by the injection of ergotine, subsequent contraction and shrinking of the uterus took place to a considerable extent. In another case of irregular menstruation and profuse menorrhagia he had also used ergotine subcutaneously, and the liquor internally, with great advantage.

*Dr Craig* was interested in Dr Cuthill's paper and his boldness in carrying out these experiments in the face of the Act of Parliament which had recently been passed. He thought it was deeply to be deplored that such a measure should have been allowed to become law. He thought ergot the best remedy in threatened abortion when the uterus was flabby and slight hæmorrhage taking place. He had recently treated a case of epistaxis successfully with ergot.

*Dr Kennedy* suggested that Dr Cuthill should continue his experiments, more especially on the human subject. He referred to the value of ergot in the bleeding from varicose veins. He had seen it used with success in such cases in Germany.

*Dr Keiller* asked if any Fellow knew of a well-authenticated case in which ergo had produced abortion. This was an important medico-legal point, and one on which he had been frequently consulted. He was disappointed with its effects on the uterus in early pregnancy. It was generally supposed that it would produce abortion, but he thought this was doubtful. He referred to a case in which a medical man was accused of giving ergot in early pregnancy for the purpose of inducing abortion, premature labour having subsequently come on, causing the death of the female. He was asked to investigate the case, and to state his opinion as to the possibility of ergot bringing on the labour. The defence was that sarsaparilla was given, and not ergot. Chemical analysis having failed to detect the difference between the two drugs, the case fell to the ground. On the whole, his experience taught that, in early pregnancy, ergot did not act with sufficient power on the uterus to produce abortion. In the latter months, when the muscular fibres were developed, and in labour, when the fibres were prepared, or were already contracting, he had no doubt of the power of ergot in stimulating contraction, and thereby greatly facilitating the emptying of the uterus and diminishing the tendency to post-partum hæmorrhage.

*Dr Craig* mentioned that Dr Stephen Wilson had performed experiments on himself with various ergotized grains without any bad results.

*Dr Matthews Duncan* had used ergot a great deal. His opinion as to its effects differed from that of many of the Fellows. He thought its action was very variable, and that we really knew very little about it. It seemed to him a remarkable proof of our ignorance of the action of this remedy, that it was recommended as of service in stopping abortion. Although not objecting to Dr

Cuthill's reference to its "energetic effects," he must say he had ever failed to see them. In his experience the effect was never sufficiently energetic. He referred to a hospital case, in which good results were believed to have been produced by the subcutaneous use of ergot. In this case he varied the injection by using simple water, without the patient's knowledge, and with equal benefit. In menorrhagia he thought ergot serviceable. He had not known ergot to produce abortion. He had not used it to prevent abortion, as in such circumstances he considered its use neither logical nor correct. If it had any action in abortion, it would be to favour it. He had used it in inducing premature labour without effect. As to its use in labour at the full time, he had not found, nor did he believe, that it increased the pains, although it might in another way hasten the birth of the child. It was most useful in controlling hæmorrhage; and in confirmation of the views he had expressed he referred to the researches of Schatz with the toco-dynamometer. Although ergot did not increase the pains, it did tend to produce permanent or tonic uterine contraction; and in this way might hasten the birth of the child, but in an injurious manner. It destroyed the intermissions between the pains, and thus produced what we wanted to avoid. The real value of the remedy was in the third stage of labour, when it assisted in keeping up uterine contraction. In fibrous tumours he would not expect benefit from its use, except as a result of increased tonic uterine action. As to the analogy between the bloodvessels and the uterus in regard to the action of ergot, he believed there was close resemblance.

*Dr Craig*, in reference to the use of ergot in threatened abortions, stated that Dr Duncan not having tried the drug in such cases, his (Dr D.'s) statement was a mere matter of opinion. As Dr Duncan had said, it was useful in bleeding, and he had not known ergot to produce abortion, and there being bleeding in early abortion, he thought it would be found of service in these cases. This was but the "logical" conclusion from Dr Duncan's own statement regarding ergot.

*Dr Bruce* had begun practice with the idea that ergot had great power in increasing the strength and frequency of the pains during labour, but after a prolonged trial he had been so frequently disappointed with the result that he seldom used it for such a purpose now, although it was undoubtedly a very valuable remedy in the third stage, and as a preventative of hæmorrhage by inducing tonic contraction of the uterus.

*Dr Farquharson* mentioned that in a herd of cows as many as twenty of them had calved about the same time from eating ergotized grass.

*Dr Duncan* thought if this were caused by ergot, such results ought to occur in all countries when the grasses were much affected with ergot.

*Dr Keiller* asked what Dr Duncan meant by saying that ergot did not increase the pains, and yet produced permanent contraction? The present point of inquiry was, Whether or not ergot induced contractions of the uterus in the early periods of pregnancy? The influence of ergot of rye in more permanently contracting an already contracting uterus could scarcely be questioned.

*Dr Duncan* replied that distinction must be made between permanent contraction and ordinary spasmodic contraction; the temporary pressure of a pain and the constant pressure of permanent contraction.

*Dr Cuthill*, in reply, said that while he could not undertake to reply to all the questions started in the discussion, he would like to express the opinion, that during labour ergot increased uterine contraction. He mentioned a case in which, during labour, he gave ergot; one single strong tetanic pain was set up, lasting some time, ultimately expelling the child. As to the *modus operandi* of the drug, he could only judge of that in a secondary way, as it was impossible to have ocular demonstration of it. Granted, however, that its action was such as he had stated, it was easy to account for all the phenomena with which it was credited. In acting as a hæmostatic, he did not think it did so chemically, in the same manner as perchloride of iron, but in some other way.

V. *Dr W. A. Finlay* communicated a paper, which was read by *Dr Underhill*, entitled A CASE OF TRIPLETS COMPLICATED WITH PUERPERAL CONVULSIONS AFTER DELIVERY.

*Dr Craig* desired further information as to the watery discharge which had escaped from the vagina. He suspected it might be urine.

*Dr Underhill* could not answer Dr Craig's question, but fancied from other observations in the paper that it could not be urine.

*Dr Gordon* stated it was well known that in certain dropsical cases large watery effusions might occur suddenly.

*Dr Underhill* remarked on the comparative rarity of triplets in primiparæ.

*Dr Simpson*, in referring to the treatment of the convulsions, thought chloral hydrate acted very much as chloroform, throwing the patient into a state of quietude. He considered Dr Finlay's case interesting in many respects. He would have liked to know the cause of death in the two girls, and more particulars in regard to the sac.

*Dr Keiller*, with reference to Dr Underhill's remarks about primiparous cases of plural births, said that he had seen one long ago in the Maternity Hospital, and that he had met with four cases of triplets in all, three of them being premature, all having distinct amniotic sacs.

# Part Fourth.

## PERISCOPE.

Interesting Case of Hernia with Traumatic Rupture and Formation of Fæcal Fistula. Reported by M. Dujardin-Beaumetz.—A man with a large scrotal hernia on the right side, of long standing, received a blow on the lower part of the tumour. Some time after an abscess formed, which opened spontaneously. Fæcal matter was discharged, and, bit by bit, a portion of intestine was protruded. When the man was admitted into hospital, the protrusion was found to consist of two portions of bowel—each with an opening—evidently the proximal and distal portions of the torn gut. From the right opening fæcal matter came, not constantly, but four or five times a day. From the left only mucus came. No fæces passed per anum. An injection thrown up the left portion was at once discharged per anum. From this last fact, from the distinctly fæcal character of what came from the right opening, and from the character of the protrusion, M. Dujardin-Beaumetz concluded that the portion of bowel protruded belonged to the large intestine. He had been unable to find any case recorded, even approaching in similarity to this remarkable case. The patient refused to submit to any remedial treatment of a surgical character.

Dr Terrillon on Sprain of the "Medio-tarsal" Articulation.—Dr Terrillon having carefully studied several cases of sprain of the foot, has noticed that, whilst sprain of the "tibiotarsal" joint is the most common and serious injury, sprain of the "medio-tarsal" articulation does occur. The latter may be alone or combined with the former; in the one case it is apt to be mistaken for the former injury, and in the other case to be overlooked. Dr Terrillon considers that neglect of this sprain often is the cause of persistent pain, and may also be the exciting cause of disease of the tarsus. He has accordingly favoured the profession with his monograph on the subject. He describes the sprain as being produced when the posterior half of the foot is fixed, and the anterior portion forcibly adducted or abducted. The symptoms are those of sprains in general. The treatment recommended is, the employment of the cold douche, "methodical and continuous rubbing," and a flannel bandage at the first. Painting with iodine is to be employed should pain persist.

Professor Dolbeau on the Treatment of False Joint.—He ascribes the cure of false joint, in cases where simple means are used, to postponement of the natural process of union. He draws a distinction between simple ununited fracture and false joint.

The former may be cured by such means as rubbing the extremities of the bones together, or irritating the space between the fractured extremities by passing in needles, setons, galvanic wires, etc.; but when there is a false joint, properly so called, there is no reliable cure but resection. Where it can be made out that the ends of the fragments are hard and eburnated, he recommends the following operation:—1. A free incision through the soft parts, so placed as not to injure important structures. 2. Oblique section of the ends of the fragments. 3. Close apposition of the sawn surfaces and their retention by metallic sutures. The section must be so planned as to bring the axis of the limb straight. 4. Immobility secured by a plaster bandage. The difficulties met with seem to have been tardy healing and abscesses, and the difficulty of removing the metallic sutures after the bone had united. (The application of antiseptic dressings, and the employment of catgut sutures would, we think, make this operation a greater success.)

PROFESSOR ALBERT BLUM ON TRAUMATIC SHOCK.—In a lengthy article in the *Archives Générales de Médecine* Professor Blum expresses as his opinion that the effect of fear and of defeat or victory on those suffering from gunshot wounds has been greatly exaggerated, and that their influence has never been proved by post-mortem examinations. We are at a loss to imagine what pathological appearances one would have to look for to verify the influence exerted on a fatal issue by such mental conditions. Professor Blum describes very fully and well the characteristic signs of shock, but, we think, errs in confining that term to those cases only where the shock is due to *considerable* or extreme violence. The amount of shock and amount of injury do not necessarily coincide, and this is shown by a subsequent statement in the paper, viz., that "the lesions found post-mortem seldom account sufficiently for the amount of shock." He dwells on the reduction of temperature in cases of shock. He considers anything below 35·5 (R.) as necessarily fatal. He thinks old persons, women, and children, are less likely to suffer from shock. Considering the fatal issue due to failure of the heart's action, he recommends laying the patient horizontally, rubbing the chest, applying heat, and giving stimulants with opium. Electricity may be very useful. He is against operating during shock, for, he says, the patients usually sink and die, and, should they recover the shock, are more liable to suffer from secondary hæmorrhage. His words are, "All active surgery should be forbidden under shock."

FISSURES OCCURRING IN LONG BONES.—An interesting paper on this subject, by Dr Hodges, appeared in the second number of the *Boston Medical and Surgical Journal* of this year.

Fissures, he says, are always the result of severe injuries, and serious results may be expected in cases where they are diagnosed. They extend usually in a spiral direction, and may be divided into those unaccompanied by other fractures, those starting from a fracture, and those accompanied by, but disconnected with, a fracture. The rarity of the first two is very great, and the difficulty of diagnosis in both cases is extreme. The typical form of the second variety is the V-shaped fracture of the tibia in which fissures extending down into the ankle-joint are present. This, constituting a most serious lesion, must be treated with very great care, and the patient must be closely watched. If the fracture is compound, amputation is indicated, statistics being given which show that attempts to save the limb are always followed by grave results. Recovery from this fracture is often followed by a persistent pain, due to deep-seated inflammation either of the bone or membranes.

## MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris, and of the Pharmaceutical Society of St Petersburgh; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

Relief of Pain by External Application of Chloral-Hydrate.—In the *Medical Examiner* for 26th October, Dr Thomas Sretch Dowse has a communication on the relief of pain by the outward application of hydrate of chloral. He states that it seems to act directly upon the nerve-matter, and subdues muscular movements by reflex inhibition of motor centres; or, he asks, "Can it be that this state is brought about by inhibition of vaso-motor centres thus influencing by reflected action the supply of blood to direct motor centres?" He affirms that in chloral, applied externally, we have a local anodyne and detergent of the utmost value. In amputation he sprays the flaps with chloral solution, with the effect of preventing local pain in the stumps, and of relieving reflex startings. Dr Dowse records twelve cases in which the local application of chloral was attended with the most beneficial results, and he suggests that in traumatic tetanus the patient should be placed in a chloral bath, that is, in a blanket wrung out of a hot solution of chloral. The solutions of chloral used by Dr Dowse consisted of 4 drachms of the drug to 16 or 20 ounces of water, either alone or combined with glycerine, chloride of zinc, or perchloride of iron.

Dr Solari of Marseilles recommends the following plaster as an excellent application in cases of neuralgia and certain nervous pains arising from exposure to cold. Over every four square inches of ordinary pitch plaster sprinkle one or two scruples of chloral.

This should be left on the part for from twenty-four to forty-eight hours; when removed, a number of small vesicles will be seen; these should be opened, and the part should then be dressed with cerate.—*Paris Méd.*, 24th August 1876.

PHYSIOLOGICAL ACTION OF NITRITE OF AMYL.—Mayer and Friedrich, in experimenting with nitrite of amyl, confirmed the observation of Filehne, that it always caused an increase in the frequency of the pulse, and they agree with him in ascribing it to a diminution of the tonus of the vagus centre. Large doses of amyl nitrite paralyze the heart itself. They agree with Lauder Brunton in attributing the fall in the blood-pressure caused by nitrite of amyl to a direct action of the drug on the walls of the vessels. After a moderate dose the respiration becomes more frequent and deeper, and after large doses it becomes flat and very slow—effects which are due to a direct action on the respiratory centre. The spasms produced by this drug are due to a direct stimulation of the brain; it does not appear to influence the spinal cord. After prolonged inhalation, animals pass into a condition in which the blood-pressure falls very low, the heart-beats and respiratory movements become slow but regular, and the peripheral nerves and muscles remain excitable.—*Arch. fur. exper. Path.*, Bd. v. p. 55.

MEDICINAL USES OF NITRITE OF AMYL.—Dr Ziegler has never found nitrite of amyl to produce dangerous symptoms, but its effects differ greatly in different persons. In angina pectoris it shortens the attack, but does not act very quickly. He found it to act very efficaciously in the dyspnœa of phthisis, and in hysteria it acts almost instantaneously. He has never found it to fail to give instant relief in cephalalgia of a nervous or neuralgic type. In neuralgia it generally acts only as a palliative. In two out of three cases of asthma it seemed to effect a cure, while in a third it afforded great relief. In one case of intermittent fever it proved efficacious after failure of quinine. In five out of six cases of chorea it yielded satisfactory results; Dr Ziegler at first gave the nitrite of amyl in doses of from three to five drops, but he now gives it "almost indiscriminately when the proper cases present themselves."

Dr Osgood has also found the nitrite of amyl of service in angina pectoris, asthma, colic, etc. In three cases of intermittent fever it cut short the chill and shortened the other stages. He says that the more he sees of the remedy the less he fears it in large doses.—*Philadelphia Medical Times*, 8th July.

NITRITE OF AMYL IN HICCUP.—In the *Chicago Medical Journal and Examiner* Dr Simon records a case of hiccup which had lasted for twenty-six hours, and was instantaneously cured by inhalation of three drops of nitrite of amyl. All other treatments proved unavailing.

NITRITE OF AMYL IN NEURALGIA.—M. Baldassare records a case of a woman sixty years of age who had long suffered from ague and severe miliary fever, and in whom violent facial neuralgia, especially affecting the molar region, supervened. For two years a great variety of means were employed to give relief, as quinine, electricity, morphia, etc., but all proved ineffectual. After she had breathed from three to ten drops of nitrite of amyl for ten days, the pain entirely disappeared, and did not recur for several months. —Quoted in the *Practitioner* for September from *Annali Universali di Med. e Chirurg.*

NITRITE OF AMYL IN NEURALGIC DYSMENORRHŒA.—Dr L. B. Edwards treats cases of obstructive and neuralgic dysmenorrhœa by placing a gelatine capsule containing three or four drops of amyl-nitrite against the os uteri while the patient is lying on her back. In a few minutes the capsule is dissolved, and a stinging pain about the part is sometimes momentarily experienced. The relief from pain is almost instantaneous, and in cases of obstructive dysmenorrhœa the menstrual discharge is soon regularly established. The treatment is not curative but palliative. As the amyl-nitrite dissolves the capsules, the latter should not be filled until needed. —Quoted in the *Practitioner* for July from the *Virginia Medical Monthly.*

NITRITE OF AMYL IN EPILEPSY.—Dr Bourneville observes that nitrite of amyl not only produces important modifications in the circulation, but also probably in the composition of the blood. It lowers the temperature in man and the lower animals. The end of an epileptic attack is announced in cases where the nitrite has been inhaled by movements of deglutition, and by nausea and vomiting. Tolerance of the drug is easily established, so that the dose requires to be increased at each inhalation. Dr Bourneville does not doubt that nitrite of amyl produces a well-marked effect in attacks of epilepsy, but he is not sure that it exercises any influence on the progress of the convulsive symptoms.—*Gazette Médicale de Paris,* 12th August.

NITRITE OF AMYL IN MELANCHOLIA.—Professor Trebaldi finds the inhalation of nitrite of amyl to be of service in melancholia, which he thinks depends on a diminished activity, with excitability of the brain. In cases where stupidity and drowsiness are prominent, he finds that it produces transient intoxication, and he often found it of especial value in cases where the patient maintains an obstinate silence or refuses to eat.—*Rivista Sperimentale di Freniatria.*

NITRITE OF AMYL AS AN ANTIDOTE TO CHLOROFORM.—Dr F. A. Bunall read a paper at a meeting of the New York Academy of Medicine on 21st September on nitrite of amyl as an antidote to chloroform. He believes that it acts by producing a direct paralysis of the vascular walls, hence causing a rapid circulation. Dr Bunall believes nitrite of amyl to be the most available of all medical agents to avert the dangerous consequences of chloroform anæsthesia.

SALICYLIC ACID AND ITS CONGENERS, *in acute Rheumatism.*—In a communication to the *Lancet*, 28th October 1876, Dr Maclagan states, as his further experience of salicin in the treatment of acute rheumatism, that the best way to get the full and speedy benefit of the remedy is to saturate the system with it as quickly as possible. To adults he gives from 20 to 30 grains every two hours, and in very acute cases he gives that quantity hourly till the pain is relieved. Dr Maclagan has found that cases of acute rheumatism treated with salicin convalesce very rapidly. The following are his conclusions as to the action of salicin on the cardiac complications of acute rheumatism :—

1. That given sufficiently early and in sufficient doses it prevents those complications.
2. That its free administration is the best means of staying their progress after they have occurred.
3. That such general treatment does not exclude the usual local measures—leeching, poulticing, etc.
4. That the beneficial action of the salicin on the heart ceases when the temperature falls to the normal.
5. That salicin is powerless to remove the effusion which remains after the fever has ceased. The advantage of salicin over salicylic acid is that it is an excellent bitter tonic, and never causes troublesome symptoms, except in some rare cases, such tinnitus aurium as results from a 2 or 3 grain dose of quinine.

At a meeting of the Therapeutical Society of Paris, 11th October 1876, Dr Martineau reported that neither in typhoid fever nor in articular rheumatism did salicylic acid exercise any influence on the temperature or pulse, a statement which was confirmed by Dr Dujardin-Beaumetz.

In the *Lancet* for 11th November a case of acute rheumatism, under the care of Dr Green of Charing-Cross Hospital, treated by salicin, is recorded. In this case the drug was given early and pushed as far as the patient could tolerate it. He frequently had headache, and twice complained of dizziness and noises in his ears. The salicin did not notably lower the temperature, nor did it control the pain. It did not prevent the recurrence of endocarditis, pericarditis, or pleurisy. It is, however, worthy of note that the perspiration, which was acrid for a day or two, became afterwards slight and unirritating, so that no miliaria appeared. In the same journal two cases are reported by Mr Tuckwell, in which symptoms of poisoning were produced by the acid. In both the immediate effect of the acid on the rheumatic symptoms was remarkable—a rapid falling of the temperature and pulse-frequency; a speedy subsidence of pain and swelling of the joints; a cleaning of the foul tongue and arrest of the sweating were observed. But in both alike, together with the satisfactory abatement of these symptoms, there came on a set of other symptoms, which may thus be summarized:—Humming and buzzing in the ears, with gradually increasing deafness; a peculiarly loud, deep, sighing respiration; a strange restlessness gradually increasing to delirium, not unlike that of delirium tremens, with involuntary evacuation of urine and fæces in the worst of the two cases; a slow, labouring pulse; an olive-green colour of the urine. All these symptoms declared themselves at the same time that a low temperature, a slow pulse, and an absence of all rheumatic appearances, showed that the disease itself was in abeyance. No sooner was the medicine withheld than the strange symptoms ceased, and the rheumatism returned. In one case the nervous symptoms were so alarming that Dr Tuckwell was afraid to use the acid treatment in the relapses, but blistered the joints, according to Dr Herbert Davies's plan.

In the *Lancet*, 18th November 1876, Dr John Curnow records four cases of acute rheumatism treated by salicin. Three of these seemed to indicate that the salicin treatment was the true desideratum in this affection; but, in one of them, it afforded no relief whatever, a very acute pericarditis supervened, and the remedy was powerless against this complication and the attendant fever.

In the *British Medical Journal* for 9th December 1876, Dr Balthazar Foster records a case of acute rheumatism in which the free use of salicin, although it reduced the pulse-rate, failed to materially lower the temperature, to prevent heart mischief, or to shorten the duration of the disease.

In the *Medical Times and Gazette*, January 6th and 13th, are recorded details of eight cases of rheumatic fever, under the care of Dr S. Ringer, treated by salicin. In these cases relief of pain was one of the earliest effects noticed. In three of them the joint-pain ceased at least twenty-four hours before the temperature began to fall, and at least four days before it became normal. In three others the temperature became normal before cessation of pain, and in two of them the pain persisted. In two cases the pain subsided, and the temperature became normal simultaneously. In only three cases did a murmur develop whilst taking salicin. This was in each case a distinct apex-systolic, and it disappeared before the drug was discontinued. In one case a murmur developed after the discontinuance of the drug. In the other four cases no murmur existed, though the soft low first sounds in two of them suggested the anticipation of murmurs. The sweating was profuse in three of the cases, produced miliaria in one of them; was alkaline in a third. In all the rest the skin was merely moist. The urine never gave any large deposits of lithates, was usually only moderately acid, and on one occasion was alkaline. Salicine was detected in the urine in one case six hours after administration, and gave the purple reaction as late as the fourth day after the discontinuance of the drug; in this respect differing notably from quinine.

At a meeting of the Clinical Society of London, held on the 12th January, Dr Hermann Weber read particulars of cases illustrating the treatment of rheumatic fever and some other febrile diseases by salicin and its congeners. The following are his conclusions:—1. Salicin and its congeners are powerful antipyretics, equal to quinine, with the exception of the latter in ague, against which they are comparatively powerless. 2. They are of more general value in the treatment of rheumatic fever than any remedies hitherto tried, although they are not applicable in all cases, and thus not always able to prevent complications. 3. Their antipyretic influence may be usefully employed in other febrile diseases, such as typhoid fever in combination with other modes of treatment, such as alcohol and bathing. 4. Their use is occasionally attended with accidental symptoms, some of those being unimportant, as noise in the ears, various degrees of deafness, giddiness, nausea, abundant perspiration, and possibly a cutaneous eruption; while others are graver, such as vomiting after every dose, delirium, and collapse, pointing to the necessity of caution and watchfulness, especially in states of weakness of the heart and exhaustion. 5. There is no essential difference in the action of the salicin, salicylic acid, or salicylate of soda, but the latter, being more soluble and more easily absorbed, exercises a more rapid effect, but requires to be given in somewhat larger doses than either salicin or salicylic acid. Amongst the two latter salicin possesses in so far the preference, as the taste is less disagreeable, and as it is more soluble than salicylic acid, which in its undissolved condition seems to be able to give rise to local irritation of the mucous membrane of the fauces and stomach.

At the same meeting Dr Cavafy read notes of a case of acute rheumatism treated by salicylate of soda, in which there was intercurrent scarlatina and a peculiar skin eruption. The case illustrated the antipyretic and antirheumatic action of salicylic acid and its salts, but was hardly favourable, as relapses occurred frequently. It was a question whether these were due to the intercurrence of scarlatina. A curious herpetic eruption broke out, and was plainly caused by the salicylate of soda. This was certainly not a frequent toxic effect of its internal administration, but seemed to be not uncommon after external application of the acid, as shown by the cases communicated to the Society last year by Mr Callender. A gradual tolerance of the drug seemed to have been established in the case under consideration, as the second eruption was not so severe as the first, and the third and fourth administrations were not followed by any eruption at all.

In the discussion which followed the reading of these papers, Dr Julius Pollock stated that he had used salicylate of soda in about twelve cases with an almost uniformly good result. He affirmed that salicin has not answered the hopes first formed of it. Dr

Althaus could not join in the same terms of commendation of salicin and its congeners. He drew special attention to the fact that they are liable to produce skin eruptions and severe intestinal symptoms. Dr Whipham's experience was that, that in cases of rheumatic fever treated with these drugs there was no relapse; he observed no difference in the result whether salicin, salicylic acid, or salicylate of soda were used. Dr Rogers obtained with salicin all the advantages of the acid without its drawbacks. Dr Broadbent concurred in the conclusions arrived at by Dr Weber in his paper. Sir W. Jenner had seen very little improvement from the use of salicin; but by salicylic acid he thought great benefit had been produced.

In the *Lancet*, 10th February 1877, a case of acute rheumatism is reported as having been treated by Dr Bradbury of Cambridge with salicylate of soda. The report of the case concludes with the remark that this was the only case of acute rheumatism which had been under Dr Bradbury's care in which the patient has recovered with a stiff joint, and it was the only one in which he had prescribed a preparation of salicylic acid or salicin.

The following are the conclusions arrived at by Dr J. W. Moore as to the use of salicylic acid in acute rheumatism. 1. It appears to be a valuable and almost specific remedy in the treatment of acute rheumatism. 2. After the administration of a few moderate doses of five grains each, given at sundry intervals, a marked amelioration of the symptoms usually occurs. Thus the temperature and pulse begin to fall, the swelling and pain of the affected joints subside, and the patient sleeps. 3. The above doses are quite sufficient to produce an impression on the disease, while the patients make but little complaint either of the frequency of the dose or of the taste of the medicine. 4. When pushed far it sometimes causes ringing of the ears and diaphoresis. Under these circumstances its administration should be temporarily suspended. 5. To prevent relapse it should be given for some days, but at gradually lengthening intervals. 6. Finally, as to its probable action as a preventive of the dangerous cardiac lesions of acute rheumatism, the author could only endorse the words of Dr Coates of Belfast:—"I think it can hardly be denied that medicines which cut short disease, as I believe there can be no doubt salicylic acid does, must render the liability to these complications less." —*Dublin Journal of Medical Science*, January 1877.

The following are M. Sée's views as to the use of salicylic acid: As an antipyretic it is inferior to digitalis and quinine, the diminution of temperature being less marked and less constant. On account of its insolubility, he prescribes it in powder in doses of about 8 grains every hour. Salicylic acid is eliminated from the system with great rapidity, and its presence may be detected in the urine within a few minutes of its injection, hence the necessity of giving it in small, frequently repeated, doses. In five cases of subacute rheumatism treated with salicylic acid, the pain and swelling ceased on the day after its administration. None of the patients had any cardiac complication. Two of them showed symptoms of intolerance of the drug, as indicated by cephalalgia, insomnia, ringing in the ears, etc. He withholds his judgment as to the value of salicylic acid until he has given it a further trial.—*Brit. Med. Journal*, 10th Feb. 1877.

Justi tested the value of the salicylate of soda as an antifebrile agent in thirty-one cases of acute articular rheumatism and typhoid fever, and arrived at the following conclusions:—In many cases, in doses of from 60 to 70 grains for children, and of 100 grains for adults, it causes rapid diminution of temperature, so that a fall of 1° to 2° C. may be observed in the course of two hours. Its antifebrile action attains its maximum six hours after its administration, and then gradually diminishes. Diminution of the pain only occurs in cases of acute articular rheumatism when the temperature has fallen at least 1° C. If the temperature continued to fall with the use of the remedy, the pain also diminished, so that the two are, he believes, proportional. The use of the salt should be continued for some days to prevent relapses, 20 or 30 grains being given daily. He found it useful in the gastric catarrh of young children.—*Deutsche Med. Wochenschrift*, No. 22, 1876.

# Part Fifth.

## MEDICAL NEWS.

Army Medical Service.—List of gentlemen who were successful in competing for appointments in Her Majesty's Army Medical Service at the Examination held at the University of London on the 12th of February 1877.

| Order of Merit. | Marks. |
|---|---|
| 1. Mullen, J. J. | 2623 |
| 2. Murphy, F. H. S. | 2353 |
| 3. De Caux, T. | 2210 |
| 4. Johnston, W. F. | 2011 |
| 5. Hodson, R. D. | 1995 |
| 6. Armstrong, J. | 1945 |
| 7. Kirkpatrick, H. C. | 1886 |
| 8. Browne, A. W. | 1875 |
| 9. Powell, C. R. | 1808 |
| 10. Hogan, E. M. A. | 1781 |
| 11. Ellis, P. M. | 1770 |
| 12. Kearney, T. | 1705 |
| 13. Kenny, W. J. | 1645 |
| 14. Irwin, A. | 1550 |
| 15. O'Sullivan, P. J. | 1521 |
| 16. McCarthy, W. | 1465 |
| 17. Brodie, J. F. | 1290 |

The *Pall Mall Gazette* is responsible for the following.—"We are asked to print the following story for the benefit of those who are in the habit of using four-wheeled cabs:—A gentleman happened to be passing a door in a certain street in London before which stood a hack cab. As he passed, an invalid, bearing on his face the eruption of smallpox, came out from the house and entered the cab.

The observer followed the vehicle as rapidly as he could, and was just in time to see it deposit its fare at a smallpox hospital. Endeavouring to ascertain the number of the cab, our informant still followed, and saw the driver hailed by a lady and gentleman, whom he vainly endeavoured to warn by shouts not to enter the cab. They took no notice, got in and drove off. Being a resolute man, the observer still followed, and was just in time to see the pair descend at the door of a house in one of the best squares in London. Having failed in his efforts either to deter the passengers or to get near enough to the cab to take its number, the pursuer now gave up the chase. Some time afterwards, being again in London, our informant thought he would call at the house at which the lady and gentleman had alighted, with the view of explaining what might have seemed his frantic behaviour in shouting to them and in pursuing the cab. He found the blinds of the house down. The master had died on the previous day of smallpox, and his wife was not expected to recover from the same malady. How the patient fared at the hospital is not known; nor, in the event of his recovery, with what feelings he would be likely to receive the information now at his service."

## OBITUARY.

PROFESSOR HENRI FERDINAND DOLBEAU OF PARIS. — This eminent surgeon died of serous apoplexy at his residence, 10 Rue Malesherbes, Paris, on Saturday, 10th March, in his 47th year; and on Monday, 12th, his remains were laid in the family grave in the cemetery of Père La Chaise. The deceased having been a Protestant, the funeral service was conducted partly at his residence, and partly at the grave, in accordance with the Reformed ritual. The attendance, which was very large, embraced many of the medical celebrities of Paris, a concourse of students, and numerous other friends. Professor Dolbeau has left a widow, a son, and two daughters.

Professor Dolbeau took his medical degree at Paris in 1856, the subject of his thesis being traumatic emphysema. In that dissertation the character of all his future work is indicated by the desire therein shown to base rules of practice on scientific principles. Among his most considerable writings, are a series of essays on enchondromatous tumours, a treatise on stone in the bladder, and a series of lectures on clinical surgery. He was the contributor of numerous valuable papers to the transactions of the *Société de Chirurgie*, of which he was president; and to the *Bulletin de Thérapeutique*, of which he was one of the editors. Dolbeau was an honest worker, and was neither a seeker after ingenious novelties, nor a man desirous of connecting his name with a thing which was new unless he was thoroughly convinced that it was likewise true, and a step in advance. We agree with those who do not think that his surgical novelties and dogmata were always sound; but we feel satisfied that they were nevertheless the results of observation, careful study, and conscientious conviction. Even his errors, therefore, command respectful consideration, and convey instruction to many, but especially to equally honest and equally able fellow-workers.

Professor Dolbeau's *method of using alcohol in surgical dressings* is not likely to gain converts in the present day, when surgeons are so successful in healing wounds without suppuration, by methods, which, though ostensibly and nominally different, are essentially the same. He might have obtained greater successes in the treatment of wounds than those he boasted of, had he simply combined with non-disturbance the minute hygienical care which he bestowed in conjunction with his pet alcoholic system. Sir James Paget has recently stated a practical truth of fundamental importance in explaining the modern diminution in mortality, and in all the troubles apt to follow operations. He says :—" No one, I presume, would venture to assign to each improvement its share in the diminution. Mr Lister's practice and promulgation of his antiseptic treatment; Mr de Morgan's use of the chloride of zinc; silver-sutures, acupressure, torsion, carbolized catgut ligatures, Esmarch's elastic bandage, a steadier belief that the healing of wounds is *a natural process that needs only to be undisturbed*:—these have been the chief means of saving life; and with them may be counted the constantly increasing care for healthy arrangements in hospitals and houses, and for cleanliness and simplicity. But I suspect another thing has been more potent than any one of these, namely, the increase of the care in watching all patients submitted to operations, and in rejecting the unfit. There has been an admirable rivalry for success: treatment after operations has come to be the most interesting subject of surgical study; and every one proposing or adopting an improvement has watched personally and closely the patients submitted to its use. The result is, I believe, that the total mortality and all the illnesses after operations of all kinds are not more than half as great as they were thirty years ago." [1]

The alcoholic method of dressing surgical wounds has often much surprised the casual British visitor to the Hôpital Beaujon. The following was Dolbeau's plan : The bleeding having been staunched, the raw surface was washed with the strongest commercial alcohol, and was then dried with fine soft linen. The cavity caused by loss of substance was filled up, and in the case of an amputation, the flaps were covered with feathery tufts of fine charpie saturated with alcohol. The entire dressings were then enclosed in an envelope of impermeable guttapercha tissue, and retained in position by a few rounds of a bandage. During the day the guttapercha was temporarily removed, and the underneath dressing moistened with alcohol. Next day, and on each following five or ten days, the entire dressings were renewed. The charpie adherent to the raw surface was carefully moistened with alcohol before removal, to prevent any oozing of blood. At the end of eight or ten days raw surfaces treated in this way are quite dry, and present a slate-gray colour. This dried-up state may be indefinitely prolonged. To accomplish permanent healing it is necessary to induce suppuration in the wound. The idea of Dolbeau was to maintain the alcoholic dryness (*sécheresse alcoolique*) till all risk of traumatic fever was past, and till the patient was sleeping and eating naturally, and had so gained strength. He then considered that the time had arrived for promoting suppuration with a view to cicatrization. Glycerine dressings were forthwith used. If the formation of pus was excessive, occasionally alcoholic dressings were employed to moderate it. Dolbeau maintained, that by following the method now briefly described, traumatic fever was prevented; and the surgeon was enabled to arrest or diminish the suppuration of wounds at his pleasure.

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[1] Clinical Lectures and Essays. By Sir James Paget, Baronet. London: 1875. See p. 76.

Professor Dolbeau's name is well known in connexion with an operation for stone which he termed *lithotrité perinéale*. The writer has repeatedly seen him perform this operation at the Hôpital Beaujon. The majority of the cases did well. A note made at the time—December 1869—of one of the most difficult cases may be of interest. The subject was an old man. The stone was large. Complete anæsthesia by chloroform having been induced, the patient was held in the lithotomy position by two assistants. The operator began by making an incision of two centimetres in length through the skin of the perineum in the median line. This incision was carried back as far as the mucous coat of the anus. He then divided the tissues of the perineum, cutting back as far as the muscular fibres of the sphincter, so as to bring them into view without injuring them. In front, he pushed the bulb of the urethra quite out of the way of the knife. With the index finger of the left hand, which served him as a director during this part of the operation, the operator now felt through the membranous portion of the urethra the groove of a staff previously introduced by the penis into the bladder. Guided by his finger in the wound, he now made a slit in the membranous portion of the urethra with the bistoury. By this slit he introduced a director specially devised by himself for this operation. This instrument is formed of six branches, which branches, by means of a very simple mechanism, can be separated so as to be parallel to one another: its diameter when closed is ten millimetres, and twenty millimetres when open. By using this instrument gently, dilating and shutting it alternately, the operator reached the prostatic portion of the urethra. He then withdrew the staff. Continuing the process of dilation in the same manner in which he began it, he was enabled to dilate the neck of the bladder, and get free access into its cavity. A straight passage was thus formed, nearly two centimetres in diameter, leading from the bladder to the perineum, situated between the bulb of the urethra in front and the rectum behind. Neither the bulb nor the rectum was implicated in this incision, but both were squeezed up. Through this straight passage powerful forceps were introduced by which the stone was crushed. By means of smaller forceps the fragments were laid hold of, and extracted. Several times during the operation Professor Dolbeau injected water into the bladder, which was retained, showing that the vesical sphincter had not lost its contractile power. There was no hæmorrhage during the operation; and after it, no dressing of the parts was required. The patient went on well for some days, when he died, after having attempted to strangle himself. The results of a very carefully performed autopsy may be thus briefly summed up. The bladder was healthy, no tear, and no trace of inflammation could be detected. The neck of the bladder was in no degree torn. The bulb of the urethra was in a healthy state, and was not involved in the incisions. The thickness of the tissues remaining between the operative passage and the bulb was more than a millimetre.

As a scientific surgeon and a dexterous operator, Professor Dolbeau was deservedly held in high esteem by the profession, by his pupils, and by the public. His lectures as professor of *Pathologie Externe* at the Ecole de Médecine were much appreciated, as were likewise his clinical remarks as he passed from bed to bed in his morning visits at Beaujon. In every branch of his profession he was thoroughly informed. Notwithstanding an occasionally rough and uncourteous manner, he was, we must suppose, a favourite with the public, for within the last few years he has had one of the largest and most lucrative practices in Paris. He was a staunch friend; and by all those who knew him intimately, he was respected and loved. Among mere acquaintances, he was popular with some and unpopular with others.

The necessity of superseding the old-fashioned nursing by nuns, universal in French hospitals, was steadily pressed by him on the notice of the authorities of the Assistance Publique. He mercilessly exposed the evils of nun-nursing at Beaujon, and demanded a staff of trained lay nurses acting directly under the orders of the physicians and surgeons. He did several things to show that he was determined to push this matter to an issue. For example, he allowed no linseed cataplasms to be employed in his wards, having discovered that the sisters tossed the old poultices, if clean looking, into the barrel where the linseed meal was kept, so that they might be made up again in new poultices. He was always at war with the sisters, and denounced their meddlesomeness, disobedience, and dirtiness.

At the beginning of the March *trimestre* of 1870, Professor Dolbeau was unable to lecture, and for some months was very ill.

On the 6th March 1870, *Figaro* announced that the professor was suffering from "diaphragmatic pleurisy, pericarditis, and pneumonia." This notice sent great crowds of anxious inquirers to his residence. Next morning the following bulletin, signed by Professor Béhier and Dr Millard, appeared in the newspapers:—"M. Dolbeau has passed a better night. Some of his symptoms have assumed a more favourable character." He made a remarkable recovery after a large quantity of pus had been removed from the pleura. The perils of the operation and circumstances under which he recovered were very unusual, as was his case in many other respects. It was published by Dr Millard in the *Comptes Rendus* of the *Société Médicale des Hôpitaux*. For some time after recovery from this serious illness there seemed to be a satisfactory restoration to health. Latterly, however, Professor Dolbeau had been remarked to be always drowsy—unable even to keep awake at the Faculty examinations. He likewise became rapidly and increasingly corpulent. On Monday 5th March 1877, he was exceedingly drowsy in the morning at the time of his usually getting up to dress. This drowsiness continued all the following day, but was moderated by remedial measures adopted by Dr Millard. On the Tuesday, he drove with one of his daughters to visit at Père la Chaise the tomb of another daughter. The drowsiness continued and passed into coma. He died in a comatose state on Saturday 10th March 1877.

Professor Dolbeau was a member of the Academy of Medicine, and a chevalier of the Legion of Honour. As a man of integrity, an admirable teacher, a distinguished surgeon, and a reliable friend, he will be long remembered and lamented in Paris.

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Dr ALEXANDER EUGENE MACKAY, R.N., Deputy Inspector-General of Hospitals and Fleets.—The death of this esteemed and well-known gentleman, which occurred at St John's Wood after a short attack of typhoid fever, on the 10th of March, will take many by surprise; but his more immediate friends were aware that his health had been declining for some time. Although only fifty-five years old, his talents, assiduity, and energy accomplished, during his career in the Royal Navy, a great amount of valuable and enduring public work. A Scotchman and Highlander by parentage, he was born in Edinburgh, where having to contend, in early life, with the *res angusta domi*, he owed much to the kindness and the judicious help of his kinsman the late Alexander Falconar, the friend and contemporary in India of Sir William Jones. At his delightful suburban residence, with its domestic amenities and well-selected library, young Mackay enjoyed the advantage of being a frequent and welcome visitor, during the period of both his general and his professional education. Having obtained his degree in medicine, he entered the navy as an assistant-surgeon in April

1844. After a year spent at Haslar Hospital, he served on board Her Majesty's ships on the East India and China Stations, and then for some time in the Naval Hospital at Hong-Kong; until, in November 1852, he was promoted to the rank of full surgeon. In that capacity he served on the Australian station, and, subsequently, as senior medical officer of the flag-ships in the Mediterranean and on the South-East Coast of America.

His medical returns attracted special notice at an early period, for their intelligence, accuracy, and fulness; so much so, that in 1857 the Gilbert Blane Medal was awarded to him by the Director-General, aided by the Presidents of the two Royal Colleges in London, who are charged with the responsibility of adjudicating that much-prized honour. On the appointment of the late Dr Bryson as Director-General in 1864, Dr Mackay—whose qualifications were by this time generally recognised—succeeded him in the important duty of drawing up the statistical reports regarding the health of the Royal Navy. These reports, year after year, characterized by the fairness and fulness with which the data provided by the medical officers throughout the service were summarized and recorded, furnish a most valuable fund of sound information in hygiene, medical topography, pathology, and therapeutics, which has already contributed, and will continue to contribute, to the welfare not of the Navy alone, but also to that of the community at large. That the blue-books thus prepared were highly appreciated by leading men in the profession, outside of the naval service, is indicated by the fact that Dr Mackay was chosen a Fellow of the Royal College of Physicians—an honour which he greatly valued. And it ought to gratify our national pride to learn that his statistical labours are in great estimation with the medical officers connected with the Marine Department in foreign countries. To the periodical literature of his profession Dr Mackay made various contributions, among which an essay on *Dysentery* may be signalized,—a disease to which his attention was called very emphatically during his Chinese experience.

Surviving friends and relatives will be ready to say that this valuable life has prematurely reached its conclusion. But they have the satisfaction of remembering the strong and loyal sense of duty and the earnest religious convictions, of early origin, which ever animated and sustained him in the performance of his daily avocations.

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## CORRESPONDENCE.

*To the Editor of the Edinburgh Medical Journal.*

17*th March* 1877.

Sir,—A letter from Mr Erskine Stuart appeared in the last number of the *Journal*, which seems to demand a reply.

The writer extols salicylic acid as an astringent, but I must say that I have tried to find out its astringent properties in vain, *i.e.*, when applied topically as a surgical dressing. Perhaps Mr Stuart thought that I considered that the acid was not astringent when taken internally, or when injected into the rectum; but if he will take the trouble to look over the paper which he criticized, he will find that I never even hinted as to its internal administration.

In conclusion, I would only wish that Mr Stuart should compare notes, as I have done, between cases treated with the acid and its salt, and I have no doubt that he will then find that the latter will bear off the palm.—I am, yours truly, ROBERT KIRK.

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*To the Editor of the Edinburgh Medical Journal.*

GREIFSWALD, 14/2/77.
LANGESTRASSE, 73.

I AM gratified to read in the December number of your Journal, page 561, that Mr Annandale has made an excision of the joint between the astragalus and the os calcis.

I have made, as Professor Hueter's representative, the same operation (which I call "resectio sub talo") in a young unmarried person of twenty-five years *in the first day of August* 1876. The disease—chronic osteomyelitis, or "synovitis granulosa," after Mr Hueter's nomenclature, dated since eight years,—was localized in that joint between astragalus and os calcis. I found through a fistulous opening on the inner side (a little below the malleolus internus) a rough bone and the talo-calcaneal-articulation in the same manner as the sinus tarsi changed into an oblong cavity filled with soft granulations and small particles of necrotic bone. After having put the patient under chloroform, I executed an incision on the inner side one and a half inch in length, and saving the periosteum, I entered the cavity. I elevated the bone and deepened with the chisel the rough parts and the joint between calcaneus and astragalus (the seat of the chronic granulous inflammation). Aiming at this, it was necessary to perform another opening on the outside of the joint. Having cleared and cleansed the whole cavity, I laid through it a drainage-tube of caoutchouc and washed the cavity with carbolic solutions, and at last with chlor. zinc solution (8 °/$_0$). Dressing "salicyljute" (Thiersch-Lister) every third or fourth day, changed under carbolic spray. No fever. After four weeks, recovery. Six weeks after the operation the patient makes trials to go with crutches. In the middle of October she was sent home, going on a stick. The two openings are closed. She wrote to me a few days ago that she is very well, but yet somewhat feeble in the operated foot. She is walking with the aid of a stick. The result is also in this case very good; but I must agree with Mr Gillespie that it is very seldom that the disease is, as we imagine, localized in that joint; and I also believe that we owe to the antiseptic treatment the good results of the operation. Especially it is my firm persuasion, that only by the antiseptic treatment we prevent the acute inflammation extending on the neighbouring joints, inprimis on the talo-crural-articulation, but also on the sheaths of the peroneal tendons—events what would be inevitable under other circumstances.

I have often seen during the last French-German war, that this event, especially the extending acute inflammation on the neighbouring parts, disturbed the progress and the effect of resections, which were performed on this or the other tarsal-bone, or on the tarsal-articulations.

With the full antiseptic treatment we have much better results of the analogous operations.—I am, with the greatest respect, yours,

Dr M. Schüller.

## PUBLICATIONS RECEIVED.

James Barr, M.B., L.R.C.S. Ed.,—On Reduplication of the Cardiac Sound. London, 1877.

Richard Fawcett Battye.,—What is Vital Force? London, 1877.

R. Bentley, F.L.S., and H. Trimen, M.B., F.L.S.,—Medicinal Plants. Part 18. London, 1877.

A. Bouchardat,—Annuaire Thérapeutique de Matière Médicale, de Pharmacie, et d'Hygiène pour 1877, contenant le resumé des travaux thérapeutiques et hygiéniques pubilés en 1876, et les formules des médicaments suivi d'un mémoire sur la thérapeutique étiologique. 37$^{me}$ *année.* Prix, 1 fr. 50 c. Pp. 270. Paris, 1877.

Peter Murray Braidwood, M.D., F.R.M.S., and Francis Vacher,—First Contribution to the Life History of Contagium. British Medical Association Scientific Reports.

Clinical Lectures on Subjects connected with Medicine, Surgery, and Obstetrics. New Sydenham Society, London, 1877.

R. T. Dugdale.—"The Jukes:" A Study in Crime, Pauperism, Disease, and Heredity. New York, 1877.

Louis A. Duhring, M.D.,—A Practical Treatise on Diseases of the Skin. Philadelphia, 1877.

Em. Emond,—Le Mont-Doré et Ses Eaux Minérales: Notice Médicale. Pp. 107. Paris, 1877.

Extracts from the Records of the Boston Society for Medical Improvement. Boston, 1876.

Christ. Fleming, A.M., M.D., M.R.I.A.,—Clinical Records of Injuries and Diseases of the Genito-Urinary Organs. Dublin, 1877.

Fownes' Manual of Chemistry, Vol. I. London, 1877.

Tilbury Fox, M.D., F.R.C.P.,—Atlas of Skin Diseases. Part 18. London, 1877.

M. P. Garnier,—Dictionnaire Annuel des Progrès des Sciences et Institutions Médicales. *Douzième année*, 1876. Prix, 6 fr. Pp. 531. Paris, 1877.

Christopher Heath, F.R.C.S.,—A Course of Operative Surgery. Part 4. Lond., 1877.

Dr Constantin James,—Du Darwinisme ou L'Homme-Singe. Paris, 1877.

Dr Lœwenberg,—De L'Échange des Gaz dans la caisse du Tympan, considérations physiologiques et applications thérapeutiques.

V. Magnan,—Recherches sur Les Centres Nerveux: Pathologie et Physiologie pathologique. Avec 3 planches, et 2 figures. Pp. 222. Paris, 1876.

P. J. Mercier,—De la Congestion Pulmonaire Rapide: De l'Œdème Aigu du Poumon, avec ou sans expectoration albumineuse, leurs rapports réciproques, leurs rapports avec la pleurisie et son traitement:—*Etude de Pathologie Clinique, Expérimentale, et Critique.* Pp. 84. Paris, 1877.

H. Alleyne Nicholson, M.D., D.Sc., etc.,—The Ancient Life History of the Earth, Edinburgh, 1877.

Proceedings of the Medical Society of the County of Kings, Brooklyn, New York, vol. ii. No. 1.

William Thomson, F.R.C.S. Ed.,—A Third Analysis of the Statistics of Phthisis in Victoria. Melbourne, 1877.

A. Trousseau et H. Pidoux,—Traité de Thérapeutique et de Matière Médicale. Neuvième edition revue et augmentée avec la collaboration de Constantin Paul. Deux tomes. Prix, 28 francs. Pp. 971 et pp. 1265. Paris, 1877.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*Lecture on the Behaviour of Bloodclot under Aseptic Conditions.*[1] By DAVID FOULIS, M.D., Lecturer on Pathology in the Glasgow Royal Infirmary School of Medicine.

GENTLEMEN,—I have chosen for my opening lecture this summer a subject which is of great importance to us, both as pathologists and as practical surgeons. The blood might almost be said to be the most important constituent of the body, for it permeates and is in intimate relation to all the rest; and both by derangements of its own composition and from the admixture of poisonous and other substances foreign to it there result numerous diseases. But it is not to these that I wish to draw your attention; it is rather to the changes which occur in the blood considered as a *tissue*, after coagulation either in or out of the vessels in cases where it is protected from the influence of putrefaction, and is not separated from the living tissues round it.

The question of the disposal of bloodclot in wounds and in internal parts has long occupied the attention of surgeons, but it cannot be said that a final decision has yet been agreed on. There are two sets of opinions prevalent, one group of observers holding that bloodclot undergoes an actual and *bona fide* organization into a form of connective tissue in certain circumstances; and the other group assigning to it a purely passive *rôle* in the organism, of which the best result is removal by absorption.

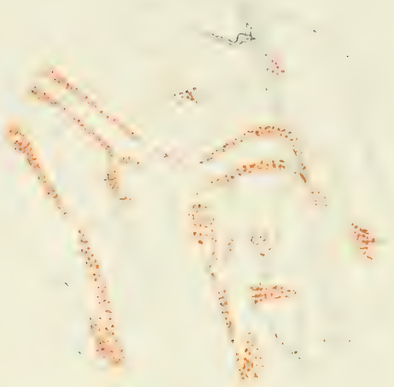
One of the leading adherents of the former view was John Hunter. You are no doubt aware that he has expressed very decidedly his opinion that the actual substance of bloodclot takes a large and direct share in the union of cut surfaces both subcutaneous and open. Now, since the time of Hunter we have learnt much about the blood. We now know that the coagulation of the blood is not a mere precipitation of a substance held ready made

[1] Delivered at the opening of the Summer Session of 1877.

as it were in solution, such as albumen, but that it is rather a formation of fibrin by the union of two substances contained in the blood separate from each other—the fibrinoplastic substance in the red corpuscles and the fibrinogenetic substance in the fluid plasma. It has been shown by Lister that the union of these two substances is hastened by contact with matter in which the vitality is from any cause deficient. From the first the clot which is thus formed marks itself out by a decided structural peculiarity. When first produced it occupies the whole bulk of the blood, but speedily it shrinks and expresses some serum—the so-called "water of coagulation" of the older writers; and this shrinking is not a mere subsidence of a precipitate, but takes effect on all sides of the clot. Examined under the microscope, the shrunk clot is seen to be composed of a fine network of clean cylindrical fibrillæ, and clear or granular anastomosing ribands and sheets. Entangled in this network are the white and red corpuscles of the blood.

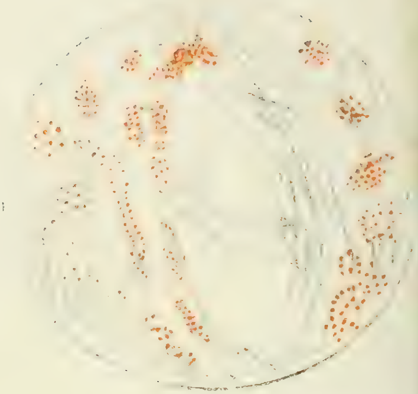
Here, then, we have a structure with decided characters. Out of the body the structure is soon lost in the changes of putrefaction; but in the body, under aseptic conditions, I hope to show you that the process of structure building does not end here, but that as John Hunter taught, it is *sometimes* the prelude to a more enduring structure, capable of taking its place among the permanent tissues of the body. I cannot better introduce what I have to say than by quoting to you a case from our own post-mortem records in support of this view. In February 1876 I made a post-mortem examination of the body of a hawker, æt. 45. The symptoms during life had pointed to cardiac disease, and no special head symptoms were noticed. At the autopsy there was found a high degree of mitral stenosis, the mitral orifice only admitting one finger; there was also aortic valve disease and thickening of the lining of the aorta. Traces of old pulmonary disease were present, but need not detain us here; the kidneys were puckered. But in the head there was a notable appearance. Over the interior surface of the right half of the dura mater there was a thin layer of a granular mottled yellow aspect; at the centre of this sheet the tissue was white and thick; at the edges the layer was extremely thin, and here it did not adhere to the dura mater, but could be floated away from it, or raised on the point of the knife like a bit of fine tissue paper. At the centre it adhered rather more firmly, but here, too, with a slight effort, it could be cleanly detached. The dura mater surface under the edges was perfectly shining and smooth; at the centre it was a very little dulled. The arachnoid over the pia mater was everywhere quite free from any attachment to the layer, and was intact and glancing. This layer of the granular aspect was removed gently, and was seen to be transparent at the edges. It was tough and strong, and at the central thicker part it had the ordinary white opaque resistent structure of fibrous tissue, like fascia. Under the microscope it presented a peculiar

A



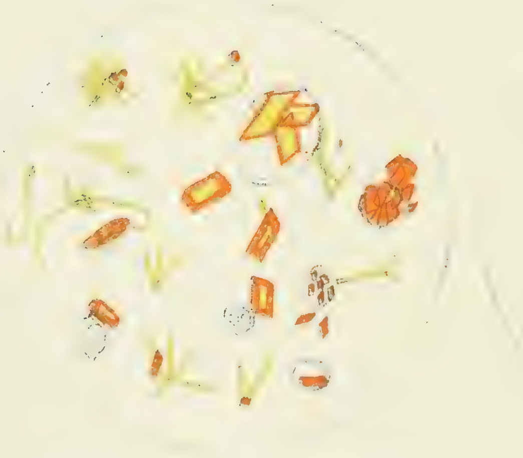
[illegible] dura mater. Harn [illegible]
[illegible] of extinct bloodvessels in the fibrous [illegible]

B



Layer of old blood clot on dura mater Hartn eye p 4 obj 6 [illegible] pigment in the fibrous structure

C



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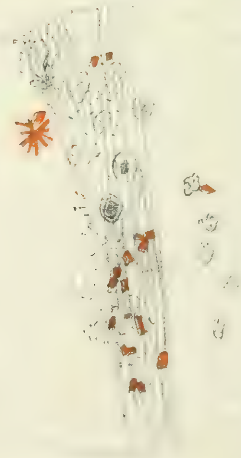
D



[illegible] 12th day [illegible]

aspect. (Figs. A and B.) Everywhere one saw a delicate and interlacing layer of wavy-lined fibrous tissue, and amid the fibrous tissue innumerable granules of a yellow colour. Most of these were round, or oval, or irregular in form; nowhere were they crystalline. In some places they were arrayed in branching lines, as if along the course of an extinct bloodvessel. Corpuscles could not be distinctly made out in any part, though here and there a shrivelled corpuscle or two appeared to exist.

You will, I think, at once admit, that we have here a tissue of a distinct degree of organization, of considerable longevity, and some interest. One asks, How could such a layer as this have originated? Similar layers are known to take origin in many cases from the cells of the dura mater—the so-called hæmatoma of the dura mater, which we shall afterwards discuss. But can we believe that a surface growth from the dura mater could become organized, and then separate so totally from the surface, leaving it as clean and smooth as before? For my part, I cannot conceive of such a thing. There must have been a degree of intimate union or connexion between all parts of the layer and the dura mater, had such been the course of events. There remains to us only the supposition that an exudation had taken place from the dura mater, and that the constituents of this exudation gradually assumed a new form, the outcome of the change being the connective tissue layer.

Now, we know that a thin layer of blood may be poured out from the surface of the dura mater, and, lying in contact with it, may become vascular. I have met with a case in which, twenty-four days after an injury to the head, such a layer of clot lining the dura mater was permeated in every direction by a fine network of blood-channels which had no walls visible under the microscope; and you will find many cases on record of a similar nature. The vessels in the clot communicate with those of the dura mater, the conditions which accompany the hæmorrhage effecting changes in the dura mater favourable to such communication. Now, in this fibrous layer from the dura mater of the hawker, we have indeed no bloodvessels; but we have traces of them so distinct in the rows of pigment granules as to leave little doubt of their past existence. It would seem, therefore, that the vessels of a vascularized clot ultimately became obliterated.

Accepting it then as certain that the organized thin layer on the dura mater was the remains of a thin layer of blood poured out on the dura mater, the question arises, What part of the clot was it which changed into the fibrous tissue of the layer? Now, some pathologists will have it that the cellular elements of the clot, or else the white cells from the blood-channels of the clot, spin themselves out into long spindle-shaped fibres, and, lying side by side, produce the fibrous appearance of the layer. This is analogous to what happens in cicatrization of granulating wounds.

But the structure of our layer is not that of a scar, not that which would be produced by such a spinning out of cells. It is, besides, opposed to what we know of the development of wavy connective tissue to resort to such a hypothesis. If we turn to Virchow's statements on the development of connective tissue, we learn (Cellular Pathology, p. 44), "that in the first place, spindle-cells and stellate-cells indisputably do exist; and, secondly, that a direct splitting up of the cells into fibres does not take place, but that, on the contrary, what is afterwards presented to our sight as connective tissue has really taken the place of the previously homogeneous intercellular substance."

Cornil and Ranvier acquiesce in this view of the origin of the fibrillar connective tissue from the intercellular substance. This intercellular substance is probably a direct product of the blood. If, then, it be admitted that these authors are correct in their views on this point, we may proceed with confidence to argue that the fibrin of the bloodclot, already of a partially fibrillated character, may further develop the permanent form of the wavy fibrillæ of the layer on the dura mater which I have been describing to you.[1] This I believe to have been the course of events.

In support of this view, I may refer you to the works of Paget (Surgical Pathology), and of Prescott Hewitt (Med. Chir. Trans. xxviii.). Paget says: "Ejection and absorption are doubtless the usual means by which blood effused in injuries is disposed of; yet I feel nearly sure it may in some instances become organized, and form part of the reparative material. The cases of manifest organization of blood, already referred to, leave no doubt of the possibility of this happening; its occurrence can no longer be set aside as a thing quite improbable. *The only question is, whether blood effused in injuries has been seen organized?* Now, I think, no one familiar with Hunter's works will lightly esteem any statement of his as to a matter of observation. He may have been sometimes deceived in thinking that he saw blood becoming organized in subcutaneous injuries (for subcutaneous granulations are sometimes very like partially decolorized clots); yet I believe he was often right, for sometimes one finds clots of blood about the fractured ends of bones which have every appearance of being in process of organization. They do not look mottled, or rusty, or brownish, as extravasated blood does when it is degenerating, preparatory to its absorption; but *they are uniformly decolorized to a pinkish-yellow hue. They have more appearance of filamentous structure than recent clots have; and they are not grumous or friable, like old and degenerating ones, but have a peculiar toughness, compactness, and elasticity, like firm gelatine.* When clots are found in this condition,

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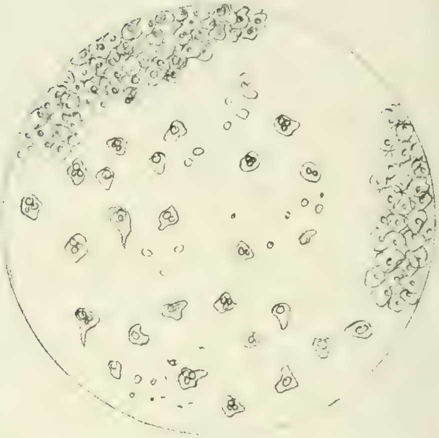
[1] Virchow draws attention (Cellular Pathology, p. 137) to the very close resemblance in the chemical and physical characters of fibrin and wavy connective tissue.

E

Antiseptic clot on open wound at 12th day, Hartn. eye p. 6. obj. 9.

F



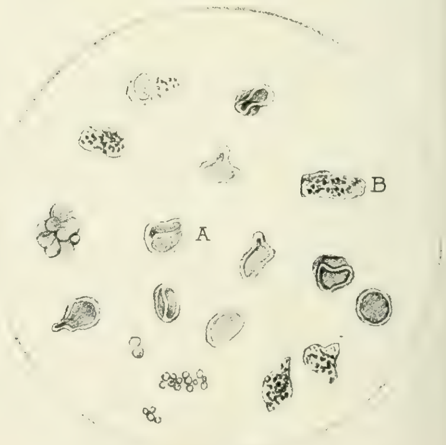
Antiseptic clot from compound fracture at 5th day The clot softening and being thrown off. Hartn. eye p.9, obj. 6.

cells shrivelled nuclei distinct

G

Antiseptic clot from open wound Compound fracture of Leg at 12th day Hartn eye p 6 obj 9

H



Cells in Antiseptic clot at 5 days, shewing shrivelled red cells, also large misshapen granular yellow cells with dim nuclei as if white cells pigmented and yellow dots and smooth granules Hartn eye p 6 obj 9

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McLure & Macdonald
Glasgow

I believe it is a sign that they were organizing, for this is the condition into which, commonly, the clot in a tied artery passes on its way to be fully organized; and (which is very characteristic) *you may find clots in the track of wounded parts thus changing, as if towards organization,* while those about them, and out of the way of the reparative process, are degenerating."

Since the introduction of Lister's antiseptic method of dressing wounds the opportunities of examining clots such as Paget describes have become much more numerous, for it is now possible to watch the changes in the bloodclots on open wounds in the living body from day to day, and even from week to week. In the wards of the Infirmary you will see numerous cases in which the antiseptically-dressed wounds are filled by clot; and, if you watch carefully, you will see that two processes appear to take place in such clots. By the kindness of Dr Hector C. Cameron, I am enabled to give you the conclusions which I have come to from a study of clots from wounds under antiseptic treatment in his wards. The first, and perhaps the more common of the two processes which I have referred to, is softening and removal of the clot. The serum and pus in the wound soak the under surface of the clot, the cells infiltrate it from the adjacent tissue, and in a day or two, if you tear away such a clot, you find that it lifts off quite easily, leaving a moist granulating surface below. The outer aspect of the clot is ruddy or fawn-coloured, and the texture is firm; the under surface is soft, gray, and pulpy, where it lay on the granulations. Under the microscope the whole clot is found remarkably cellular, the fibrillar structure having nearly disappeared; in a few spots traces of yellow pigment are visible. The cells are very irregular in outline (Fig. F), crenated, rounded, or oval, and the contents are granular, the granules dark and distinct. Nuclei are commonly single, but may be double, or even triple—changes which are probably due to the acidulation of the fluids by the carbolic acid.

But there are clots which give quite another picture. At the fifth day these clots are tough and well preserved, the colour is ruddy fawn-colour. The blood-corpuscles, under a high power, are contorted, and there is a degree of diffusion and separation of pigment in round clear granules (Fig. H). The white corpuscles are not spindle-shaped, the fibrin preserves its fibrillar structure.

At the twelfth day the clot is tough, pale, salmon-coloured, and firmly attached to the surface below it. With a little trouble its fibrillæ can be teased apart like tow, and then, under a high power, the fibrillæ appear as in Figs. D, E, G. The blood-cells are still distinguishable, many of them breaking up, but none of them spindle-shaped or tending to spin out into fibres. There are usually no vessels in the clot: though it adheres firmly to the surface of the wound it does so merely as a scab does; but in certain cases you may observe, especially if the clot be in a sinus surrounded by vascular tissue, that vessels or blood-channels appear just as they do in the layer of blood on the dura mater.

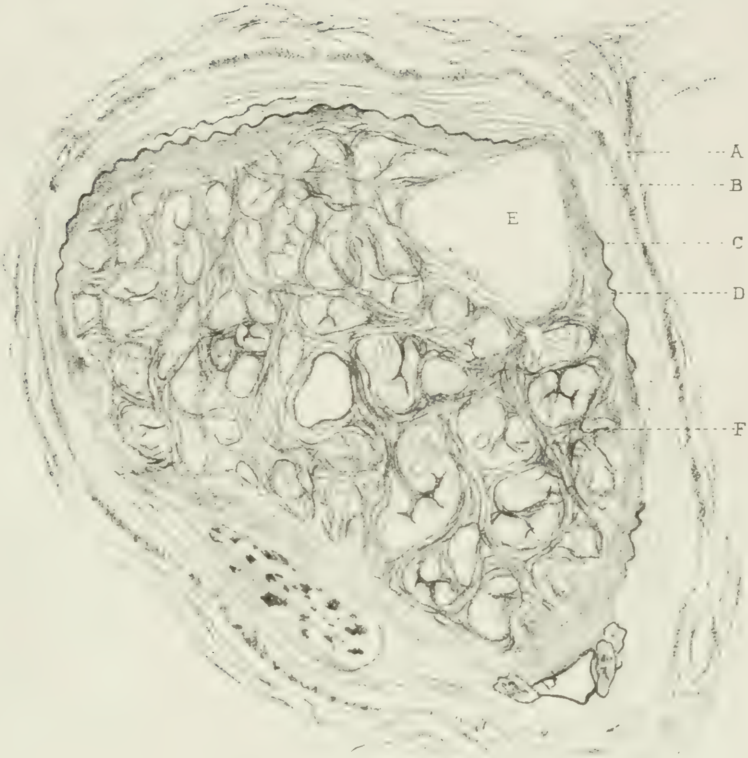
From the end of the first week onwards crystals of hematoidin are to be seen in profusion in the clot, crystals of varying size and outline (Figs. C and D). The needle form and the rhomboid crystals are the most common types, but there are sometimes peculiar crook-like or curved crystals with branching ends, probably owing their form to chance aggregation of smaller straight crystals.

What is the interpretation of these appearances? It cannot be that the carbolic acid acting upon the clot emphasizes the fibrillar structure, for the action of that acid on clot out of the body tends to render it crumbly and friable. I think we may rather learn from this study that bloodclot is naturally capable of slowly condensing, and so of assuming the form of the connective tissue (as in the layer on the dura mater) when maintained in contact with the fluids of the body, and protected from putrefaction; and when there is vascularization of the clot there seems to be no doubt that in certain instances this kind of organization really occurs, and that the fibrin takes its place as part of the connective tissue. But on open wounds the function of the bloodclot in aseptic conditions is rather that of a more or less moist scab, protecting the healing surface beneath it.

Let me bring under your notice another phase of the persistence of bloodclot in the body. The drawing which I show you (Fig. M) was taken from the microscopic view of a cross section of the femoral artery in a patient who was under the care of Dr Macewen in 1875 with aneurism of the right femoral.[1] Under treatment solidification of the aneurism took place. A year later the patient died of cancer of the tongue, and I examined the body. The aneurism was converted into a solid fibrous nodule; and the femoral artery below the aneurism was as far down as the popliteal space, full of a firm pale clot. In this clot there ran a fine channel, into which a thin probe could be passed. A cross section of this plugged artery, under the microscope, gave the appearance which I have tried to portray on the diagram. The coats of the artery are, as you see, thickened and diseased, and these diseases will occupy us later on in the course. Confine your attention in the meantime to the plug in the artery, which is composed of a clear uniformly-reticulated fibrin adhering to, but quite distinct from, the internal coat. A few scattered grains of pigment are visible in the fibrin; corpuscles of any sort are rare. Here, then, we have a substance, presumably a year old, in the process of slowly shrinking in the interior of a bloodvessel and not derived from the wall of the latter which is clearly distinguishable. The firm plug is what is known as a *thrombus*, and it is the remains of a once soft red bloodclot. The corpuscles and

[1] Glasg. Med. Journal, January 1876, p. 132.

**M**



Cross Section of Femoral Artery.

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the pigment have all but vanished, and the condensed fibrin of the clot alone remains. How long it would endure in its present condition it would be difficult to say; it has, at all events, survived the immediate metamorphosis of the clot, and forms a part of the living body, liable to be affected by its diseases and by its changing conditions.

I have now shown you three phases of the existence of bloodclot in the living body. I should be sorry to lead you to infer that the *rôle* of the bloodclot is a very high one in the formation of tissue; but I hope that I have convinced you that it has certain imperfect tendencies in that direction, and that its existence may be indefinitely prolonged in the body under favourable conditions.

## ARTICLE II.—*Two Cases of Puerperal Pleuropneumonia.* By ANGUS MACDONALD, M.D., F.R.C.P.E., F.R.S.E., Lecturer on Midwifery and the Diseases of Women and Children, Medical School, Edinburgh.

(*Read before the Obstetrical Society of Edinburgh*, 14*th March* 1877.)

THE following cases of singular interest occurred in my practice in the latter part of last year. As they were observed with a considerable amount of care, and as I cannot but think they are not of very usual occurrence, and as I believe they might serve for encouragement to some medical brother in charge of patients similarly affected, I have deemed it advisable to publish them.

It is to be noticed that though the one occurred in September and the other in December of last year, and though the first was entirely better long before the other took ill, so that there could be no relation of common origin in the cases, yet they present a wonderful number of points in common in regard to symptoms and course, the principal difference being that the second was the shorter of the two, and did not present symptoms of phlegmasia alba dolens, which retarded so seriously the convalescence of the first patient. With these preliminary remarks, I proceed to record the cases.

### CASE I.—*Pleuropneumonia. Phlegmasia alba dolens. Recovery.*

Mrs P. L., Edinburgh, æt. 26, was confined by me of her second child on the evening of the 10th September 1876. The child presented the vertex with the occiput backwards, and to the right sacro-iliac-synchondrosis, and the labour was extremely easy. Though I lived close to my patient's house and was sent for early, when I arrived the head was well down in the pelvis, and one good pain effected forward rotation of the occiput, and completed delivery immediately after my arrival. The child was a healthy female, and did well. Mrs L. did well also till the evening of the 14th September, when she had a rigor after some undue exertion. Still she passed a good night, and felt better in the morning. On the 16th and also on the 17th, her elder child was unfortunate enough to meet with a slight tumble on the stair, and on each occasion Mrs L. was greatly, though quite unnecessarily frightened. In the course of Sunday (the 16th) Mrs L. began to suffer from pain on the right side, just below the nipple. This at first was slight, but as it increased on the Monday (the 17th), I was sent for in the evening, as I had not thought it necessary to see her during the day.

I found her suffering much from the pain in the right side, as also breathless and pale. Temperature was 101°, respirations 40, and pulse 110. There was pain in the right shoulder, and the characteristic symptom of the pleuritic stitch. There was a demonstrable amount of dulness at the right base, both before and behind, more so behind than before, with feeble breathing, but no increase of vocal resonance, and neither crepitation nor friction. The lochial discharge was red in colour, plentiful in amount, and free from any disagreeable odour. The tongue was foul. Urine rather scanty, but it contained no albumen. I ordered a poultice to the side and a Dover's powder to be taken.

*18th.*—Patient very much as on the previous night. No friction to be heard. Temperature, 101°·8; pulse, 110; respirations, 40.

*19th.*—Patient passed a poor night, but feels slightly easier. Pulse, 110; temperature, 101°; respiration still 40; and pain in the chest slightly diminished. Expression of countenance anxious. Cough very slight. Dulness posteriorly decidedly increased, but no friction nor crepitation audible. In the course of this day the patient spat up a mouthful of red expectoration, and in the evening the temperature rose to 103°. The poultices were continued. Another Dover's powder was administered, and the patient slept a little better than on previous night.

*20th.*—Respirations, 36; pulse, 100; temperature in morning, 101°·5; in evening, 102°·5. Expectoration characteristically prune-juice in colour, mixed with some gelatinous rusty sputum. It comes up with tolerable ease and in considerable quantity. Cough still slight. Pain on inspiration still present. No distinct friction murmur audible, though I heard what sounded suspiciously like a scrape once or twice below the right nipple. The dulness behind is markedly increased, passing up as high as the upper angle of the right scapula, and is very absolute. But the level of dulness does not appear to be altered by change in position. The dulness anteriorly is difficult to determine with precision, as the mamma is gorged with blood and also tender. But, so far as can be made out, it is not extensive. The breath-sounds are extremely feeble and short over the region of dulness. But there is no distinct crepitation ordinarily audible, though now and then I was convinced I heard a distant crackle. Neither is there increase of vocal thrill nor of resonance. No tubular breathing can be heard. The breathing on the left not markedly puerile. The same treatment continued. Diet light and no wine.

*21st.*—Had a rather better night. Physical signs of right side about same as yesterday, only I heard with tolerable distinctness some crepitation over an area on the right back, bounded externally by the lower half of the right scapula, and internally by the spinal column. Slight increase of vocal resonance is also perceived there.

Morning pulse, 92 ; respirations, 33 ; temperature, 102°·2.
Evening „ 96 ; „ 30 ; „ 101°.

Dr Matthews Duncan saw the patient at my request this afternoon. He thought he heard an indication of friction over the spot in front where I had previously thought I had heard it. We agreed to apply a blister (6 × 8) over the right side, and to give a diuretic containing five grains of nitre and a teaspoonful of Mindererus's spirit in each dose. There was some irritation of the urinary organs from the cantharides, though I had taken the precaution to insist on my patient drinking plenty of fluids during the night. There are positively no uterine symptoms to be detected.

*22d.*—Patient feels on the whole better.

Morning pulse, 96 ; respirations, 30 ; temperature, 99°·3.
Evening „ 90 ; „ 28 ; „ 99°·4.

*23d.*—In the morning, pulse 84 ; respirations, 34 ; temperature, 99°·4. The evening temperature, etc., not taken. Tongue more clean. Appetite a little better. Expectoration free. The sputum chiefly of the prune-juice character. Uterine discharge still red and copious, but it has no special fœtor. There is now crepitation audible along the whole of the right back over the region of dulness, and there is also a certain amount of increase of vocal resonance.

*24th.*—Pulse, 84 ; respirations, 30 ; temperature, 99°. Patient feels better. Dulness posteriorly absolute as high as the upper angle of right scapula, and it is considerably above the upper limit of the liver in front. Milk returning to the breasts. Expectoration still bloody, free, and copious ; comes up without much effort. There is a soft systolic mitral murmur distinctly audible at both the base and apex of the heart. I then left for the country, and my friend Dr Playfair took charge of her in my absence.

On the *27th* he reports as follows :—The front of right chest is clear on percussion, with an occasional râle. Posteriorly, the state of the chest is much as reported on the 24th. But on the left side, posteriorly, a dull patch is found to exist, stretching from the extreme base to a height of about three inches. The percussion note is peculiarly high pitched, and wooden in character. On auscultation, no fine crepitation is audible anywhere, only an occasional coarse râle with inspiration. Vocal resonance slightly increased over the dull area. Sputum still of the prune-juice colour. Perspiration is copious. Tongue, except at tip and edges, coated with a brownish fur, but it is moist. Appetite fair. Bowels moved twice to-day after taking a piece of Tamar Indien.

*28th.*—Had a tolerably good night, but sleep very broken. Complains of stiffness of left leg and pain in the left groin. The leg is not swollen nor tender, only a slight swelling can be felt in the groin, and this is tender on pressure. Sputum now mixed with mucus and not so red. Diet to be very light.

*29th.*—Improving on the whole. Pain in groin and stiffness of leg as yesterday. Lochial discharge has increased in quantity, and is of a brighter red colour than for days past. Sputum darker, and more mucus in it. Ordered two glasses of claret per diem.

*2d October.*—Left leg swollen to about twice the size of the right; pain in the groin continues. The femoral vein is felt to be hard, swollen, and very painful on pressure, and along the inner surface of the thigh the internal saphenous is found to be similarly affected. Along the calf of the leg the branches of the internal saphenous are also felt to be hard, swollen, and painful, but not so much so as the vessels higher up the limb. The general condition of the patient does not seem to have been much impaired by the onset of the phlegmasia, her health being if anything improved, except that she does not sleep well at night. The leg was now ordered to have a lotion of lead and opium applied to it constantly, and to be wrapped in cotton-wadding from the knee to the groin. A mixture of quinine and iron was also prescribed to be taken thrice daily.

*3d.*—The flat wooden note has entirely disappeared from the bases of both lungs. On auscultation the respiratory murmur on the right seemed rather louder than natural, but no crepitation nor rhonchus audible, and vocal resonance does not appear to be increased. On the left side, about the middle of the lung, the breathing is remarkably bronchial, and accompanied by coarse crepitation on inspiration. Vocal resonance over this area is much louder than on the right side. Above and below the middle of the lung the vocal resonance and the respiration approach the normal, but would seem if anything rather louder than in health. The sputum now almost entirely composed of mucus, with only a slight intermixture of blood.

*8th.*—I again took charge of my patient. I found the condition of the chest nearly normal. Not a trace of pain or other disturbance in the uterine region, but still a considerable amount of sanguineous discharge from the vagina. There is also some blood in the expectoration, but no moist sounds are audible in the chest. Left leg is painful. The veins of the groin are hard, and the leg slightly swollen. Pain also is complained of on pressure along the course of the large vessels in the thigh. There is great pain on pressure on the right groin, the veins feel corded and knotted, but there is little swelling of this leg. Pain is likewise complained of when pressure is made along the course of the vessels; but there is no enlargement of the superficial veins of this leg, and no elastic œdema. The legs are being fomented frequently, then rubbed with oil and laudanum, and wrapped in cotton-wadding. The tonic mixture and wine are being continued.

11*th.*—Condition very much as at last report. It is noticed that the soft systolic murmur still persists, being heard both at base and apex. It is, however, more distinct at the apex than at the base. The left leg is nearly free from pain; the right leg is in the same condition as on the 8th. Discharge from vagina has nearly ceased. No uterine tenderness. Patient sleeps better.

13*th.*—Right leg in region of thigh considerably swollen and painful on pressure, also œdematous at the ankle. Left leg quite well. Patient ordered 10 minims of the tincture of the muriate of iron and 5 grains of the chlorate of potash every six hours.

15*th.*—Much improved.

19*th.*—Patient feels very comfortable. Tongue is cleaning. Lochial discharge slight, and now yellowish-white in colour. Swelling in right leg now quite gone. There is still a little tenderness on pressure in right groin, and along the course of the large vessels in the thigh. Ankle is free from œdema. Appetite good.

2*d November.*—Patient able to be up. Expectoration still dark. She has, however, no cough.

29*th December.*—Mrs L. still thin. Lungs both normal. Slight blow at apex of heart with the systole. Prescribed cod-liver oil for her. She has no cough, and eats tolerably well.

13*th March* 1877.—Mrs L. continues to do well. The first sound in mitral area is slightly muffled and prolonged, but there can scarcely be said to be a murmur present. The pulmonary second sound is somewhat intensified.

### CASE II.—*Pleuropneumonia. Recovery.*

Mrs B., æt. 36, was attended by me during her second confinement on the 3d December 1876. Her first labour occurred on 12th November 1875, and on that occasion I had to terminate the case instrumentally, and needed a considerable amount of traction effort to clear the head of the bony outlet.

On the present occasion I was called at 7 A.M., and found that my patient had been in tolerably severe labour since 2 A.M. The os was fully dilated, and I ruptured the membranes immediately after my arrival and gave chloroform. The head presented in the right oblique diameter of the pelvis, with the occiput posteriorly and towards the right. The head had nearly, but not quite, come down to the floor of the bony pelvis, and the anterior fontanelle was very markedly lower than the posterior. In the course of the following two hours, however, under the influence of tolerably powerful pains, the occiput was pushed down and rotation of it forwards effected; and instead of, as before, too much extension of the head existing, an abnormal amount of flexion was developed. Being convinced by this mechanism that the outlet of the pelvis was abnormally tight for the head, also finding that the latter was not making satisfactory progress, I applied forceps, and, after one or two pretty strong pulls, succeeded in drawing the head through the outlet. I noticed that the posterior parietal bone had been pushed over the anterior in the line of the sagittal suture. The child was a healthy female, and did well.

On the night of the 4th of December, Mrs. B. felt a stitch immediately below the lower border of the false ribs on the left side posteriorly. I saw her early on the morning of the 5th. Her expression was decidedly anxious. She complained of a catching pain on inspiration in the left side, which, as compared to the right, was rather fixed. Auscultation could only detect the slightest suspicion of friction occasionally. There seemed to be a shade of dulness at the extreme base, and pain was complained of in the left shoulder. Pulse was 96; temperature, 99°; respirations, 22. Skin moist; tongue clean and moist at edges—dry in the centre. There was no history of any rigor, nor of exposure to cold. Lochial discharge natural. No abdominal tenderness. In the evening the respirations were 22; pulse, 120; temperature, 100°·4. Poultices were applied constantly to the left side.

*6th.*—Loud dry friction audible at left base, both anteriorly, posteriorly, and laterally. Milk in the breasts. Tongue and skin moist. Pulse, 120; temperature, 100°·4; respirations 30, in the morning. Pulse, 124; temperature, 100°·8; respirations 28, in the evening.

*7th, Morning.*—Perspiration copious; sudaminal eruption. Friction at left base less audible, but dulness much more pronounced. Still it does not reach as high as the lower angle of the left scapula. Breathing somewhat bronchial, and vocal resonance increased. An occasional crepitation heard beyond the friction sound. No expectoration. Vocal fremitus not increased. Temperature, 100°·2; pulse, 120; respiration, 30.

*Evening.*—Had motion by means of enema. No cough nor vomiting, but a mouthful of bloody sputum was expectorated. Temperature, 101°·8; pulse, 124; respirations, 46.

*8th, Morning.*—Dulness up to level of lower angle of left scapula posteriorly. Friction loudly audible over dull area, and crepitation distinctly to be heard beyond the friction murmur. Breathing bronchial, vocal resonance increased over the area of dulness. On the right side an occasional crepitation heard in front. The patient was not turned round to examine the right back. The first sound of the heart at the base is distinctly muffled. No albumen. Temperature, 103°; pulse, 132; respiration, 36. A large blister ordered to be applied over the left side. It rose well. The patient was at the same time ordered to take an alkaline diuretic, and to have mild, non-stimulating food, but no alcoholics.

*Evening.*—Feeling decidedly anxious about my patient, I asked Dr Matthews Duncan to see her with me. We then found a slight degree of extra consciousness of pressure over the uterus, which, however, did not by any means amount to tenderness. Physical signs very much as in the morning, only that, on turning round the patient to the left side, an occasional crepitation, with bronchial breathing, and some increase of vocal resonance, was heard at the right base. No friction was audible on the right. The heat-rash was very general in extent, mottled, not unlike that of scarlet fever, chiefly confined to the trunk, and not affecting the vulva. Lochial discharge still normal. Dr Duncan recommended, in addition to what had been done, a tablespoonful of castor-oil and a teaspoonful of turpentine. This gave rise to two copious stools during the night. Temperature, 103°·4; pulse, 132; respirations not counted.

*9th, Morning.*—Slept occasionally during the night. Looks easier. Enjoyed some toast and tea this morning. Chest symptoms not worse. Temperature, 101°·6; pulse, 132; respirations, 34.

*Evening.*—Dulness at left base not increasing. Some crepitation audible at right base, where there is also dulness and increased vocal resonance. Temperature, 101°·6; pulse, 120; respiration, 28.

10*th.*—Chest symptoms as yesterday, only crepitation at right base posteriorly more distinctly marked. Patient slept six hours. Cardiac systolic murmur more loudly heard at the base, and rather harsh and grating in character. At apex (mitral area) a boom is heard with the first sound, but no murmur. The second sound is clear. No increase of cardiac dulness. The cardiac murmur is suspiciously like friction. Bowels moved unassisted yesterday afternoon, and again to-day. Patient feels decidedly better. Is taking ten drops of turpentine thrice a day in water.

*Morning.*—Temperature, 102°·8; pulse, 125; respirations, 24.

*Evening.*—Temperature, 102°·4; pulse, 120; respirations, 24.

11*th.*—Had a very good night. Bowels slightly loose. Last night a mouthful of bloody sputum expectorated. The expectoration is usually tinged with blood, but is slight in amount, and there is scarcely any cough. Friction nearly quite gone from the left side. The dulness is also diminishing. Crepitation is still audible. Cardiac murmur persistent.

12*th.*—From this time the patient improved slowly, but uninterruptedly, as a glance at the accompanying chart will show. The signs of pleurisy and pneumonia gradually disappeared from both lungs.

The cardiac murmur, which, from its character and situation, led me to more than suspect the existence of pericarditis, gradually lost its harsh character, and developed into a very soft systolic blowing murmur, which was propagated in the direction of the inner third of the right scapula, forcing on me the conviction that it had been all along, in great part at least, endocardial.

On the *29th December* I have the following note:—Patient doing well, and is to get up to-day. The systolic murmur is most loudly audible in the second intercostal space close to the left edge of the sternum. It is soft but distinct, and *is audibly* propagated along the course of the aorta and great vessels. It is audible at the lower edge of the inner third of the right clavicle, but not under the middle third of the left clavicle. It has now entirely lost the creaking and grating character it formerly possessed.

The two cases above related are the only specimens of this kind of lesion that I have been called upon to deal with in the course of practice. They both presented symptoms at once serious and threatening. The first gave me a great amount of anxiety in case the illness would turn out unfortunately; and the second, I must also confess, caused me great, though, compared with the first, less uneasiness. As I already hinted, it was the conviction that the record of these cases, if published, might prove a solace to some member of our profession, who might be called upon to deal with his first case of the kind, that prompted me to read them before this Society. I cannot think that conditions such as we find in the patients before us, possessing as they do so many special points in common, do not occur now and again, though I have not been fortunate enough to fall upon them either in practice or in reading. Dr M. Duncan, who was kind enough to see both cases with me, has stated to me that he has met with other two of late having a considerable resemblance to them. The one, however, was a case of pleurisy only, the other was one of empyema, complicated with perimetritis. The first case recovered. The second is still living, but in a very doubtful condition.

Let me now endeavour to trace the leading peculiarities of the cases.

We had nothing very special in respect to the labours. The delivery in the first case was remarkably easy. In the second, it could hardly be called severe. I merely terminated the case with forceps to save my patient what I considered quite an unnecessary amount of suffering. Her previous labour had been rather difficult and severe, and yet nothing untoward happened. I have drawn attention to the mechanism in her case merely as a point of interest to the exact obstetrician. My patients were in perfectly comfortable circumstances, possessing every necessary means to enable them to avoid exposure to cold or other injurious influences, both at the time of the delivery and afterwards; and the nurses were trustworthy, careful, and intelligent, so that nothing in the management of the patients contrary to the strictest rules of caution and propriety could have occurred. Besides this, both patients were in good health at the time of their confinement. There was the slight influence of fret, on account of the elder baby having a first and then a second tumble, which was dwelt upon by the attendants of the patient in the first case. But, then, we know that patients and their friends are very apt to place too much importance upon the influence of trivial incidents such as these on the causation of disease. And, besides, there was no such cause even thought of in the second of the two cases. It is therefore extremely difficult, if not impossible, to find any likely cause for the seizures. In both cases the pain came on at first slight, and gradually, though rapidly increased in severity. There was no sudden seizure, with fainting and dyspnœa, in either case. I examined with very special care, in regard to the first patient, whether any sudden disagreeable feeling had arisen, when, on the first occasion of alarm about her child, it appears she sat up in bed. But I could elicit nothing. She at that time did not feel faint, she did not feel pain in the side nor in the abdomen, and she was not in the slightest degree breathless. In fact, she noticed no bad effects from this premature effort of exertion.

I mention these particulars, because my mind was strongly inclined to predicate the possibility of pulmonary embolism, if the facts were such as could be squared into that idea. But I fail to find sufficient grounds for such a belief, although I have sought very carefully for them. As negativing embolic disturbance, I ought to mention that there was no antecedent varicosity in the legs of either patient—there was no inconvenience in the lower limbs other than a little of that passive œdema of the legs that one almost constantly sees towards the end of pregnancy, and there was no evidence of any metritic mischief in either of the cases. On the other hand, the onset and course of the chest symptoms were very unusual. The friction, in the first instance, was extremely ill-defined, and the rapidity with which the dulness increased was very remarkable. The dulness in both instances was very absolute, and did not appear to be due to the effusion of serum, but to an exudation of a more solid element. At any rate, change of position seemed to make very slight alteration in its level. Pneumonia accompanied, if indeed it did not precede the pleurisy in both cases, and yet even it did not exhibit the ordinary phenomena associated with this disease. The vocal resonance was slowly and imperfectly developed. There was little consonance of the breathing, and there never was fine crepitation heard. Indeed for days no crepitation of any kind was audible, and then it was distant and specially large for pneumonia. But the most peculiar point in regard to both was the presence of sanguineous sputum. The cough was in neither case much, and the mucous expectoration was slight. But in both cases mouthfuls of bloody expectoration were frequently spat up without effort. In the second case this was slight in amount, but in the first case it was copious and long-continued, the prune-juice colour of the sputum forming a marked feature in the case, and the colour of the expectoration remaining darkish-red for months after the severity of the illness had passed off. There were thus very many points that could be explained on the assumption of embolic infarction of the lungs more readily than on any other consideration. For this would explain the bloody expectoration, the situation of the commencement of the lesion in both cases at the extreme base of the lungs, the sudden supervention of dulness without at first corresponding fever, and the defective amount, as well as the retardation of the appearance of the crepitation. The ultimate height attained in both cases by the temperature, about 103°, also would fall in with the embolic theory. But, on the other hand, the absence of dyspnœa, the gradual onset of the attacks, the want of evidence of any metritic mischief or of inflammation in the right side of the heart, and the favourable termination of the cases, to my mind rather oppose that view, so far as my reading and observation of such cases carry me. At any rate, if the cases were due to embolism, the plugs must have been of a purely healthy character, and could scarcely have been the result of any pelvic phlebitic mischief, but must have simply been the product of coagulation in the uterine venous sinuses.

In both cases there was evidence of slight endocarditis, affecting in the first case the competence of the mitral valves, and in the second producing a certain degree of aortic obstruction. I am really at a loss to determine whether in Mrs B.'s case there was not a certain amount of pericarditis superadded to the endocarditis, as the murmur was so creaking and harsh, and so strictly confined to the base. But be that as it may, there was undoubtedly a certain degree of endocarditis affecting the semilunar valves, as evidenced by the permanent obstructive murmur at the aortic orifice.

From the observation of cases like these, as well as in consequence of certain researches on the bearing of heart disease upon pregnancy, with which I am at present engaged, and the results of which I hope to lay before the Society at an early date, I have been led to believe that the puerperal condition is apt to act very prejudicially on the vascular system generally, and I have been brought to doubt whether it is really rare to have endocarditis set up in connexion with the lying-in period. I am very strongly inclined to suspect that, though luckily ulcerative puerperal endocarditis is a rare disease, a slight amount of endocarditis in the puerperal state may be common, and has not been often observed, because seldom looked for. At any rate, since I met with the above cases, I have seen a third, certainly, and I think, although I am not so positive in regard to it, a fourth case, in which there existed slight and transient endocarditis, as evidenced by some cardiac irregularity and feeling of discomfort in the cardiac region, with a loud, or at least distinct, blowing systolic murmur at the base. I do not mean by that the soft mitral systolic of the lying-in period, so much talked of by German obstetricians, who, I am convinced, however, exaggerate its frequency of occurrence. My opportunities as yet have not been sufficiently great to warrant me in doing anything more than merely throwing out a suspicion regarding the frequency of slight non-pyæmic endocarditis in the puerperal condition.

That, however, the vascular system was generally in a condition prone to inflammatory change in Mrs L.'s case is evidenced by the supervention of phlegmasia alba dolens, with its usual concomitant of corded and enlarged veins, and of tenderness along the course of the bloodvessels of the affected limb. That venous thrombi originating in a limb affected with phlegmasia alba dolens may occasion, in the first instance, pulmonary infarction, and lead then indirectly to pleurisy, pneumonia, and other results of an inflammatory nature in the lung, is abundantly insisted upon by Virchow, Trousseau,[1] Gerhardt,[2] and a host of other observers, so that I need not refer to this point.

But the credit is due, so far as I have been able to make it out, to the elder and younger Begbie of having pointed out that phlegmasia alba dolens is not unfrequently either an antecedent or consequent of pleurisy, the swelled arm or leg appearing on the side on which the pleuritic lesion was situated.

Dr Warburton Begbie[3] says, in speaking of four cases of acute pleurisy in which he performed the operation of thoracentesis: "In one of those four, a young gentleman of twenty-four years of age, the pleural effusion was associated with a peculiar swelling of the corresponding limb, both in the leg and in the thigh. This swelling was not of an œdematous character, but firm, and resembling a good deal the condition of the extremity when affected by phlegmasia alba dolens; but Dr Begbie informs me—and the observation appears to be very interesting—that in three cases of pleurisy occurring to him within a limited period of time, he has observed the swollen limbs corresponding to the pleuritic side. In one of those, the lymphatic disturbance in the leg—for such it would appear to be—preceded the pleurisy; in the others was consequent on it."

I am further informed by Dr Matthews Duncan, that he has distinct recollection of phlegmasia alba dolens following an attack of acute pleurisy in a young unmarried lady, a patient of his. He also knew of the same sequence of events occurring in a young gentleman, a relative of his own. But in this case the pleurisy itself arose in the course of convalescence from an attack of typhoid fever. As phlegmasia alba dolens is not an uncommon sequela of typhus fever, of course the value of this example is somewhat less than if it had occurred independently of the latter affection.

It is, however, evident, that there is some occult connexion subsisting between pleurisy and phlegmasia alba dolens even under conditions that exclude the possibility of pyæmic processes, or any form of blood-poisoning.

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[1] Clinical Medicine, vol. v. p. 331.

[2] The Hæmorrhagic Infarction, German Clinical Lectures, second series, p. 264.

[3] Edin. Med. Jour., vol. ix. p. 1095, June 1866.

That we had here such a case in Mrs L., I think is undoubted, for I cannot believe that either case can be fairly considered as metritic. At the same time, I can hardly avoid believing—in fact, I ought rather to say, I am very strongly convinced—that the puerperal condition had a very special influence in modifying the course of the chest affections.

I can only, by reference to the peculiar condition of the blood that is special to pregnancy and the lying-in period, and perhaps also to the necessary dorsal decubitus associated with the latter, explain the peculiarities of the pleuritic and pneumonic processes in these cases, such as the rapidity with which the semi-solid dulness was developed, the absence of minute crepitation, the slowness with which the crepitation appeared, as well as the sanguineous sputa, except upon the idea of pulmonary embolism. To the latter view I am strongly inclined, could I see convincing proof of its being the correct one.

But be those theoretical notions what they may, it is abundantly evident that severe pleuro-pneumonia may arise from causes not necessarily resulting from puerperal fever within the childbed week; and though they perhaps will always give rise to serious concern, they are not necessarily of evil omen, if the other symptoms exclude childbed fever.

Of course the difficulty will always be to exclude the latter affection.

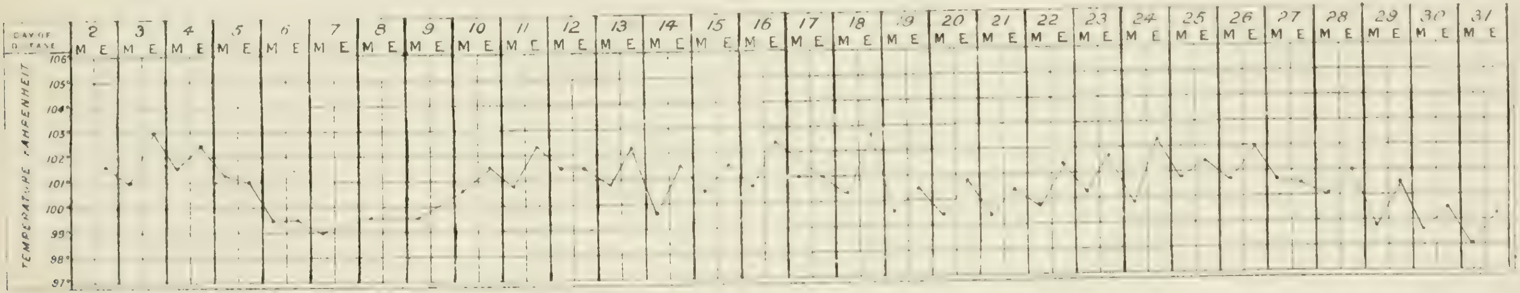
A single word as to the temperature. It will be observed on inspecting the accompanying charts that it ran its course on somewhat lower lines than ordinary pneumonia would have done.

In the first case it ran up to 103° on the evening of the third day. From the fourth to the seventh it declined. It began to rise on the ninth, when the left lung became affected, and again showed signs of declining from the thirteenth to the fifteenth, when it rose with the occurrence of the phlegmasia. With the disappearance of the acute symptoms in the left leg, it again oscillated upon the line of 100°, till the right leg in its turn became affected, when it rose to 102°·3 on the twenty-fourth day, after which it steadily and persistently declined.

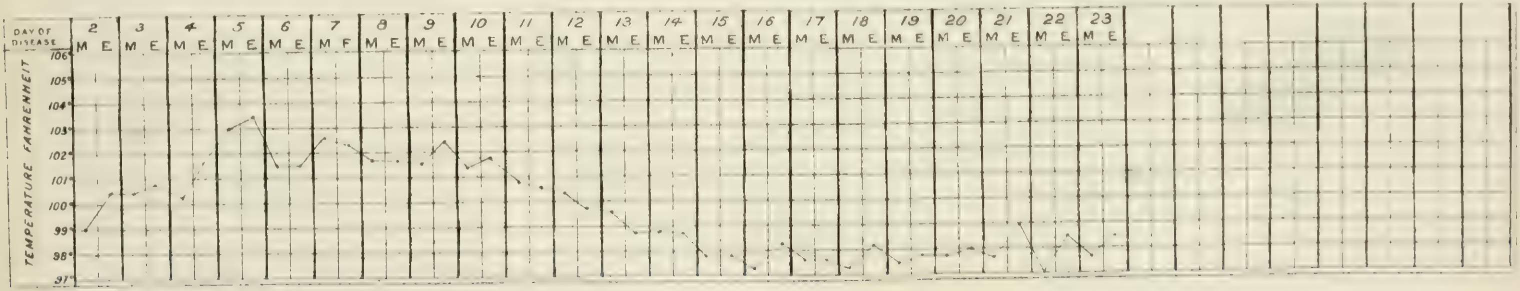
In the second case it reached its acmé on the evening of the fifth day, when it rose to 103°·2. It then continued much about the same for four days, when it declined steadily, but very gradually.

As to treatment, I have really nothing to say. It was purely symptomatic. I think the blisters did good in both cases. I also think the chlorate of potash and the iron were of benefit in the first, and the turpentine in the second case. But one dare not dogmatize in such matters.

I. Chart of Temperature in Mrs L.'s Case.



II. Chart of Temperature in Mrs B.'s Case.



*Supplementary Note.*—On the whole, after carefully re-reading Gerhardt's article upon pulmonary infarction, referred to in the body of the paper, I am disposed to believe that simple pulmonary embolic infarction is not so distinctly excluded as I was led to suppose at the time I read this paper before the Society. I find particularly that the local signs in my cases coincide very closely with the local signs as given by Gerhardt at p. 272 *et seq.*, and especially so in so far as the local signs in my cases were *peculiar.* I am therefore led to believe, that the assumption of hæmorrhagic infarction arising from embolism and leading to pneumonia and pleurisy after all explains the case better than any other supposition, and is not absolutely negatived by any evidence in the case, as I had supposed. Still, there are several difficulties in the way of its acceptance, more especially from the absence of any urgent symptoms at the commencement of the illness, and the want of any positive evidence of metritis or of pelvic phlebitis. I cannot think that the phlegmasia in the first case was related to the chest affection in the relation of cause and effect, appearing as it did after the chest symptoms had almost entirely disappeared. Nor is there any evidence that both conditions were due to a common cause, as the most careful examination of the abdomen did not reveal any metritic or phlebitic mischief from whence both might have arisen.

At all events, the embolic plugs, if such existed, must not have been of a septic nature. It certainly takes a considerable amount of faith to believe that embolism may have arisen from the escape of a simple antiseptic coagulum from the placental area, or from the uterine sinuses in connexion with two such uncomplicated cases of labour happening so near one another; and yet this assumption seems to me after all to meet the requirements of the case better than any other supposition.

## ARTICLE III.—*A Case of Lymphadenoma (with Leukemia?); great Enlargement of a Gland in the Anterior Mediastinum; Enlargement of the Liver, Spleen, Kidneys, and Lymphatics, in a Patient whose Twin-sister died of the Same Disease; Death; Necropsy; with Remarks on the Etiology and Treatment of Lymphadenoma.*

By ALEXANDER MORISON, M.B. Edin., Honorary Medical Officer to the Holloway and North Islington Dispensary, London.

I HAVE been induced to publish this case, not only because we have to do with a very interesting example of a disease which has, within a comparatively recent period, engaged the attention of pathologists, but also, as it seems to me, to contribute a mite towards the solution of the very obscure, yet practically most important, question of its etiology.

Ada B., seven years of age, and very fair, had always enjoyed tolerable health, and had never suffered from any of the infectious diseases of childhood. Her usual residence was in the north of London, but she spent some time every year in the country. She had always had as much food as she wished, and was particularly partial to milk and eggs as articles of diet.

The patient consulted my friend Dr Black of Canonbury on the 14th of March 1876, when she had an enlarged cervical gland close to each ear, was quite free from any constitutional excitement, and, though not ruddy, was not by any means markedly pale. I have been unable to ascertain particulars of any further examination of the patient at this date. She was ordered syrup of the iodide of iron, and sent to the country. While there her appearance did not satisfy her relatives, and a local practitioner was consulted, who saw the cervical glands were enlarged, but was not struck by anything farther in the child's appearance. When he next visited the patient, it was pointed out to him that her clothes would not meet so as to button, and that her abdomen was decidedly enlarged. She was immediately ordered home, and her mother, who was sent for, on her arrival, at once recognised the similarity of the child's condition to that of a twin-sister, who had died three years previously, of what Dr Andrew Clark is said to have called leucocythemia.

On the 22d of April 1876, Dr Black again saw the patient, whom he found feverish, and with a distinctly enlarged liver and spleen, in addition to the enlarged cervical glands. She also bled from the mouth, had a very fetid breath, and passed large quantities of urine, which soon became milk-white.

*Family History.*—The patient was, as I have stated, one of twin-daughters. Her twin-sister, Florence B., was, like herself, very fair, and, in fact, resembled her so closely that it was difficult to distinguish between them. This sister, when four years old, without previous illness, and having, like herself, never had any of the so-called diseases of childhood, ran against a lamp-post, striking her head violently on the right side, but without sustaining any scalp wound. A week after this accident, not having felt well in the interval, she began to exhibit convergent strabismus of the right eye, and, some days later, the mouth is said to have been drawn to the same side, rendering it difficult to retain food in it. A blister was applied behind the right ear. The paralysis, as it was termed by the physician in attendance, persisted for a fortnight, when it disappeared; the patient left her bed, and for two days seemed to be on the highway to health. Her appetite, which, without being great, had always been fair, fell off after the receipt of the head injury, and never recovered itself. After the two promising days mentioned, her strength appeared to give way almost suddenly, and she was again put to bed. The abdomen was soon observed to enlarge, and this enlargement was explained by an increase in the size of the liver and spleen. Two patches of ecchymosis also appeared in the skin of the abdomen, and enlarged till their diameter measured about three inches. Three or four days before death the cervical glands on both sides enlarged, and, in the words of her mother, she was "wasted to a skeleton," being even more emaciated at death than the sister who died last. The urine and fæces retained a healthy appearance throughout, and the former was never, as in the present case, milk-white. There was no post-mortem examination, and the blood was not examined during life.

The other surviving members of the patient's family—one boy and seven girls between the ages of eighteen and three years,—are all fair, but none so fair as the twins. They have, on the whole, enjoyed good health, and, besides the usual diseases of children, the only affections they have suffered from, have been, sore throats and enlarged cervical glands; the latter have occasionally been so much inflamed as to threaten suppuration, but this has never actually occurred. All would be called strumous.

The mother of the patient, who is now forty years of age, is of much the same type as her children, and, when younger, suffered from what she calls a "delicate chest." She has borne eleven children, and miscarried seven times within seventeen years. The period of her pregnancies at which she aborted was usually about the fourth month, and in one instance at the sixth month; the cause assigned for these accidents being some imprudent over-exertion. The twins were born of the tenth pregnancy, and seventh delivery at full term. While *enciente* with them she was in a more than usually exhausted condition, having only one year and a half previously borne another child, which she suckled herself from the time of its birth, till she was four months pregnant with the twins. She always nursed her children herself, and generally for about sixteen months. For some weeks before the birth of the twins she was so much exhausted as to be confined to bed. When these were born, the one who died at the age of four years was the smallest, and appeared to be the most delicate, she therefore herself nursed *her*; the other who survived longest (and the patient in the present instance) was nursed by a healthy country girl. This wet-nurse occasionally, though rarely, also suckled the other child. Three children have been born since the twins, two of whom are alive, ruddy, and apparently in good health. The third died during parturition, having been a "face presentation."

The patient's father has frequently suffered from rheumatism, but has never had rheumatic fever. Otherwise, he has enjoyed fair though never rude health.

I have also carefully examined the history of disease in the patient's family for four generations, and find that her maternal grandmother, who is alive, sixty-three years old, and apparently in good health, has suffered for a number of years from an old *empyema;* that her maternal grandfather died, aged fifty-three, of a lung disease resulting from *bronchitis;* that her paternal grandfather died of *brain-softening* when sixty-five years old; that a paternal grandaunt died of *cancer;* and that no relative on either side of the house is known to have suffered from consumption, joint, or bone disease, or any other manifestation of struma. The circumstances of the family are said always to have been comfortable.

*History of the Attack continued.*—I myself saw the patient on the evening of 30th April 1876, when she had just had, for the first time, several severe rigors, and had become much excited, being semi-delirious, exhibiting much dyspnœa, and giving vent to a short, sharp, *cry.* During the fortnight preceding this date she is said to have complained much of a sense of suffocation, and had, in consequence, to be propped up when in bed. The pulse was 192, and very weak; the temperature 105°·2; the skin dry and hot, the surface waxy white; and on the abdomen numerous pale petechiæ. The cervical glands on both sides were much enlarged; the lips cracked, rather swollen and dry, except that they were blackened, and, to some extent, moistened by a little blood, which seemed to come from the gums; the tongue appeared to be clean, but was likewise smeared with blood. This blood had at first been naturally florid, but latterly became very pale. There was great thirst, and the stomach was so irritable that almost all food was rejected,—milk as a solid curd, together with flakes of brown mucus. The fæces were of a dark colour, probably owing to her having taken syrup of the iodide of iron throughout her illness. The liver extended to within two inches of the umbilicus; the splenic dulness was also increased, passing down into the left lumbar region. The abdomen generally was distended and tympanitic. There was frequent inclination to micturate, and the urine, when passed, was rather smoky, but otherwise clear; on cooling, however, it became milk-white.

As the patient appeared to be moribund, I did not make any further examination. The character of the urine, as noted on 29th April, was as follows:—"Quantity examined, about ten ounces, which was voided on one occasion. Reaction acid (I find I have unaccountably omitted to note the specific gravity). Colour that of rich milk, perfectly opaque, the whole of which opacity disappears on heating. Slight cloud of albumen. The microscope shows amorphous urates, with here and there a red blood-cell, and some pavement epithelium." The urine was allowed to stand for some days, and on examining the deposit, a few crystals of uric acid and numerous hexagonal plates resembling *cystine* were observed.

Next day I saw the case with Dr Black, who pointed out to me that the apex of the left lung anteriorly was dull on percussion. This was very distinctly so, and the dulness extended down as far as the fourth rib. Respiratory murmur in the area of this dulness was diminished as compared with that on the right. The heart-sounds were distinct, and, now and then, I thought I detected a basic bruit, but, owing to the rapidity of the heart's action, could not determine whether it was systolic or diastolic. The child appeared to be quite conscious. In addition to the abdominal petechiæ, which were more numerous, there were two larger patches of extravasation over the right gluteal region; some blood still escaped from the mouth. The patient's general condition was much the same as on the previous night, except that she was less restless and weaker, and the tympanitis had so increased as to mask the *percussion* dulness of the abdominal organs.

Her temperature was 105°·6, and pulse 180. She died the same evening, was perfectly conscious to the last, quite free from convulsion, and had only been absolutely confined to bed for one week.

*Thirty-seven hours after death I examined the body*, which was decidedly, but not extremely, emaciated. Rigor mortis was well marked everywhere, except in the neck, where it was not at all evident. There was a very thin layer of subcutaneous fat on the chest and abdomen. The cervical glands on both sides were considerably enlarged. On raising the sternum, a large glandular body, in the position of the thymus, passed from the level of the sternal ends of the clavicles down to the lower margin of the second rib on the right side, and to that of the third rib on the left side; superiorly, the body narrowed so that the rounded apex of a cone passed out of the chest, up the front of the trachea, to the lower margin of the thyroid gland, from which it was distinct; the inferior boundary of the mass was a gently curved line, passing from the lower edge of the second right rib, about the costo-cartilaginous union, to the same point of the lower margin of the third left rib. The growth passed backwards to the posterior mediastinum, pushing the great vessels before it. In consequence of this the lungs were widely separated, and the pericardium over the heart and large vessels exposed. That over the vessels was so intimately connected with the gland, that, in carefully dissecting it away, the pericardium was pricked, and a straw-coloured fluid escaped from its cavity. The left lung was pushed upwards and backwards, and was adherent here and there by strong bands to the costal pleura, diaphragm, and pericardium. The right lung also adhered to the diaphragm. Both lungs were crepitant, and floated in water; the right was the larger and heavier of the two, but floated best. The surface of both was more uneven than is usual. On section, the tissue looked condensed and felt indurated. The bronchial glands were only slightly enlarged.

The heart weighed 4¾ ounces. Over the margin of the right ventricle there were what seemed to be varicose veins, but which, *in situ*, resembled circumscribed patches of extravasated blood. This appearance was probably a consequence of obstructive pressure by the enlarged gland before mentioned. Right auricle, normal; tricuspid valve, normal; right ventricle, slightly hypertrophied; pulmonary semilunar valves normal, as also the pulmonary artery. The right auricle and ventricle contained *white* coagula. Left auricle, normal; mitral valve, normal; left ventricle, somewhat hypertrophied; aortic semilunar valves healthy, but rather pouchy, as though they had been subjected to increased pressure; aorta, healthy.

The liver was much enlarged, its capsule and section smooth, and the latter peculiarly pale. The spleen weighed $12\frac{1}{4}$ ounces, and was firm; its capsule was smooth, and it had a well-marked notch in its anterior border. Its section also was smooth and clean, the Malpighian bodies not pronounced, and the test for amyloid degeneration negative. The kidneys were remarkably enlarged; the left weighing $11\frac{1}{2}$ ounces, and the right $11\frac{3}{4}$ ounces; their capsule peeled off readily, showing a generally smooth and very pale surface, with considerable patches of extravasated blood; their substance had a soft and doughy feel; the cortex was much increased; the pyramids greatly congested, and blood in them extravasated; the pelvis was normal. Some of the mesenteric glands were considerably, others only slightly enlarged. The lumbar glands were not increased in size. Other parts, so far as examined, were healthy.

Blood found in the body after death showed a decided increase in the number of the colourless corpuscles; but the red cells were undoubtedly most numerous. There were clots in both the right heart cavities, that in the auricle being largest. Some of the great veins also contained coagula. All were, without exception, decolorized, and not only so, but had a milk-white appearance; some were mixed white and red, but the former hue preponderated.

## MICROSCOPIC EXAMINATION OF ORGANS.

*Intestine.*—A longitudinal section of the small intestine was normal in every particular.

*Mesentery.*—A transverse section of the mesentery, between the small intestine and a slightly enlarged mesenteric gland, was likewise normal.

*Mesenteric Gland.*—A mesenteric gland, only slightly enlarged, showed the usual appearance of lymphatic gland structure, with hyperplasia of the cell elements, the lymphoid corpuscles invading at places the interstitial tissue of patches of adipose cells.

The portions of intestine and mesentery, and the mesenteric gland examined, were in direct communication.

*Growth in the position of the Thymus.*—In a section passing from the circumference towards the centre, that portion nearest the circumference shows a quantity of well-marked, elastic, fibrous tissue; and here the hyperplasia of cell elements is little marked, this being merely the capsule of the gland. Below the capsule there appear to be a number of canals, or spaces, not filled with blood. Next we come to the gland texture generally, which shows an enormous hyperplasia of the cell elements, embedded in a fine branching stroma, traversed by numerous delicate capillaries. Near the circumference of the growth there are sections of a few large bloodvessels, having the irregularly angular lumen of veins, In the delicate capillaries blood-cells are seen, some colourless, others coloured, but the proportion of the one to the other I could not determine exactly; in some the red cells appeared to be decidedly the most numerous. Besides the usual small, rounded cells, there are larger ones with a varying contour, a granular cell substance, and several nuclei, the latter resembling very closely the small cells of the general hyperplasia. These do not appear to be merely superimposed upon the larger cells, but to be embedded in their substance. These larger cells are not to be confounded with the cross section of a capillary bloodvessel, and seem to me to be related to giant cells, the "Riezen zellen" of the Germans. In the capsule and immediately below it there are disconnected, dark-coloured masses, of a more or less elongated shape, which appear to be sections of larger vessels. In the gland-substance there are also spaces, the walls of which are formed by rows of lymph cells; such spaces are more or less round, and vary somewhat in size. The appearances described are very similar to those met with by Dr H. G. Sutton in Dr J. Risdon Bennett's case of Hodgkin's disease with enlarged thymus, etc., in vol. xxii. of the Transactions of the Pathological Society of London.

The larger cells above alluded to, and met with here and there, might be explained as arising from an aggregation of the smaller lymphoid cells,—a mode of origin for giant cells, which I suggested in a paper on bone absorption, published in the *Edinburgh Medical Journal* for October 1873, and which, probably, also explains the presence of giant cells in tubercle. Ziegler, assistant-pathologist in the University of Würzburg, in the *Centralblatt f. d. Med. Wissenschaften* for October 1874, relates how he succeeded in forming giant cells from colourless blood-corpuscles—an analogous cytogenesis.

*Spleen.*—Lymphoid cells crowd every part of the section; the fine stroma in which they are embedded is more defined than in the other organs examined. A few bloodvessels appeared to me to have somewhat thickened coats, and were surrounded by a clearer interval between them and the substance of the organ, which was filled with the same lymphoid cells. Other vessels were normal. Delicate capillaries were likewise seen to run through the mass of cells, and by the interlacing of their branches to form spaces, which contained the cells. Some of the cells in these spaces had the configuration of red blood-corpuscles, as also had some of those in the capillaries. The appearances in this organ seem to indicate a mere proliferation of its normal pulp.

*Heart.*—Normal. The hypertrophy of this organ, though slight, was probably a consequence of pressure upon the capillary system by the deposit, together with the enlargement of the parts affected, and was doubtless also due to the compression of the great vessels by the enlarged mediastinal gland.

*Lungs.*—New growth, consisting of cells of the same kind as elsewhere, is thickly deposited, not only around the smaller bronchi, but equally abundantly around the air-cells in the fibrous septa, dividing one cell from another. The sections examined were taken from the apices of both lungs.

*Liver.*—New growth, consisting of small, rounded cells, infiltrates the liver, like other organs, and is disposed thus:—It is most abundant at the periphery of the lobules, in the neighbourhood of the interlobular branches of the portal vein, from whence it spreads towards the centre of the lobule in the intercellular texture (tela-conjunctiva) of the organ, which to me seems somewhat thickened. In some instances the deposit was distinctly most abundant at the periphery of the lobule and around the central (hepatic) vein; while it was more scanty, though still abundant, between these points. The liver-cells appear perfectly healthy, except where there is a superabundance of new growth, when they are obscured and probably atrophied. No portion of any lobule in the sections examined was free from infiltration by the new growth. The cells of the growth itself are, as a rule, nucleated, and generally have but one nucleus. A few liver cells containing fat drops were seen, but no distinct vacuolation, such as that described by Dr Murchison (Pathological Transactions, 1870) was observed. The portion of liver examined was near the free margin of the organ.

*Kidneys.*—The interstitial tissue is much increased, and consists of an extensive and copious deposit of small, round cells, colourless, and of the same character as elsewhere; besides these there are also a smaller number of red blood-cells here and there, as if extravasated. The Malpighian bodies, though they vary in size slightly, are perfectly normal, and their capsules entire. The uriniferous tubules are also normal, and lined with healthy epithelium, but are separated more than normally by the above-mentioned small-celled infiltration. The new growth appears to be most abundant where interstitial tissue naturally exists in greatest quantity. At some parts of the section, where the new formation is less abundant, or has been removed, capillary bloodvessels are seen to form the walls of rounded spaces. In the kidneys more cells having the configuration of red blood-corpuscles (extravasated) were seen than in any of the other organs. The cell elements were teased out of a section and examined, with and without the addition of dilute acetic acid; three kinds could be distinguished—*a.* renal epithelium; *b.* red blood-cells; *c.* leucocytes. The last were both nucleated and un-nucleated; the majority of the former had but one nucleus; a much smaller number had two. The appearance of a section of the kidney is very much like that met with at an early stage of interstitial nephritis.

The powers used in examining the sections were 60, 90, 300, and 400 diameters of an English microscope. Some sections were stained with carmine, and most were clarified with glycerine.

*Remarks.*—It is much to be regretted that the blood was not examined in either of these cases during life. The enlarged lymphatics and abdominal organs, together with the increased temperature, petechiæ, bleeding gums, and generally pallid surface, were good evidences of lymphatic leukemia when these events declared themselves; but, good as they were, a microscopic examination of the blood would not have been superfluous, for cases have been met with in which most of these signs have been observed, but in which, nevertheless, there has been no increase in the number of colourless corpuscles; the pyrexia and increase of white cells, seen in blood found in the body after death, serve, however, with the other pathological conditions, to class the present case as one of true lymphadenoma with leukemia.[1]

Histologically the diseased conditions here differed from what is usual, in that the new formation was not deposited in circumscribed areas, but was more universally distributed in the interstitial tissue of the parts affected—following, in fact, the distribution of the capillary bloodvessels and their companions the deep lymphatics. This, however, can only be regarded as an exaggeration of the process more commonly observed.

*Etiology.*—The few remarks which I have to offer on the etiology of the disease I would submit merely as suggestions, and as applying to the cases under consideration.

Given that a woman of strumous diathesis marries a man who suffers from rheumatism, and is the son of a father who died of brain-softening with paralysis; that six children are born and enjoy fair health, but that all belong to that type in which one might expect struma to manifest itself on any occasion; further, that the children have been born in quick succession, and that the mother has, moreover, been weakened by miscarriages and over-lactation; that a seventh pregnancy occurs under these debilitating circumstances, and results in the birth of twins; given further, that these are of the same type as the other children; that one of the twins dies of lymphadenoma, after a severe shock to the nervous system, followed by loss of appetite, etc.; and that, three years later, the other falls off in health, without any evident exciting cause, and likewise dies of the same disease, which, in both cases, runs a rapid course, in the first instance ending fatally in a fortnight, in the last in two months; and finally, that within six years after the birth of the twins, three children are born singly, two of whom survive in apparently good health, the third having died during parturition: I would ask, Whether these facts do not support the idea which has been broached, for example, by Dr Payne (Pathological Transactions, vol. xxii.), of a close relationship between lymphadenoma and struma? Had a strumous mother who could bring *one* ovum at a time to maturity, producing a child of merely a strumous diathesis, failed under debilitating circumstances so to nourish *two* ova, the result being a development of the latent diathesis into wide-spread disease of the same type? Or, had the rheumatic taint of the father any influence on this result? For patients affected with lymphadenoma have been known to be rheumatic.

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[1] As *post-mortem* examinations of blood are supposed to be unreliable, the presence of leukemia in this case will doubtless be considered by many open to question, notwithstanding the corroborative collateral evidence.

In the case of a young lady, aged 19 years, now under my observation, and who has suffered for the last fifteen months from true lymphadenoma of the most extensive character, without any ascertainable implication, so far, of the larger parenchymatous organs, the point of departure from general good health seems to have been an attack of rheumatic fever at the age of eleven years, immediately subsequent upon scarlatina. But here, too, a strumous element is introduced, inasmuch as the patient's paternal family history is decidedly phthisical.

Notwithstanding the fact that almost all the larger lymphatic chains in this case are much enlarged, the character of the blood is at present perfectly normal as regards the proportion of red to colourless corpuscles. In a field of two to three hundred red cells only three leucocytes were observed. The blood is said to have been equally normal some months before I had an opportunity of examining it. Finally, was not the disease, in these cases, *constitutional* rather than acquired? The mere fact of twins dying of the same rather unusual complaint lends probability to this view, and it is not, I imagine, unreasonable to suppose that, but for the severe head injury sustained by Florence B., which, I presume, merely hastened the outbreak of the disease, she might have survived longer than she did, though, perhaps, not so long as her sister Ada B., for we must not forget that the latter was suckled from the first by a strong woman, while the former was nursed by her debilitated mother.

As bearing upon the etiology of the actual anatomical changes, I would emphasize the following of the conditions observed:—

1. That the new growth is invariably deposited in the interstitial tissue of the organs affected, except in the case of the lymphatic glands, which normally consist of cells resembling those of the deposit, embedded in a delicate stroma.

2. That the cellular infiltration bears, especially in the case of the kidneys, not only the features of an extraneous deposit, but also those of a proliferation of the connective tissue of the affected parts, resembling that seen in the early stage of inflammation,—a resemblance which, in the case of these organs, is strengthened by the extravasation of red blood described, and which was probably a consequence both of venous obstruction due to pressure by the

new growth, and it may be, in part, of inflammatory *vis à fronté;* these events being analogous to the arborescence and extravasation met with in some forms of nephritis.

That inflammation is a result of the deposit is also argued for by the thickening of the tela-conjunctiva in the liver, seen in this case, and also described by Dr Murchison in vol. xxi. of the Pathological Transactions.

The hypertrophy of the liver was probably due to this inflammatory thickening, in addition to the leukemic deposit.

It is well known that the doctrine of a migration of leucocytes from the vessels into the tissues has been mooted by pathologists in explanation of the morbid appearances in such cases. For example, by Rindfleisch (*Handb. d. Path. Gewebelehre*, p. 161, Leipzig, 1873), who says, "What we see is a migration of blood-corpuscles into the loose connective tissue surrounding vessels which borders upon, and may indeed go on to, hæmorrhage." He likewise, in the same place, explains the bleedings which occur in leukemia as due to "the greater friction which leukemic blood must exercise upon the walls of the vessels, on account of the preponderance of viscid colourless cells." But, admitting that many of the cells of the deposit are extravasated, we again find ourselves on the horns of a dilemma as to the vessels from which this extravasation has taken place. Is it from the capillary bloodvessels, from the deep lymphatics, or from both? The distribution of these is so much the same that this point is not easy to determine. In the liver, the new growth is on the whole deposited most abundantly at the periphery of the lobules, that is, in the principal area of lymphatic distribution. But, in the case under consideration, there is, as described, in some instances, an excess of deposit around the hepatic as well as the portal veins, and a goodly quantity in the intervening space. In the other organs a preference for lymphatic territory is not so evident, because this is indistinguishable from the distribution of the capillary bloodvessels. It is probable that the new formation is primarily derived from both these sources. From the bloodvessels by extravasation, due to an altered quality of the blood, or, if we may betake ourselves to the nervous system, aptly styled the "refuge of the destitute," to faulty innervation of the vessels, or to the combined influence of both these hypothetical agents. Contributions from the deep lymphatics may be a result of their engorgement, possibly due to obstruction in the enlarged glands, as has been suggested, if I mistake not, by Dr Moxon of Guy's Hospital.

If, then, we grant a migration of leucocytes from one or both of these sources into a gland or glands to be the first event in these cases, may we not suppose that the subsequent enlargement is a result of irritation, that is, of a degree of inflammation consequent upon this extravasation; and that, during such enlargement, a greater quantity of colourless corpuscles are drafted into the blood, accounting for any leukemia present, and, by making their way through the delicate walls of the extreme vessels, in ever-increasing numbers, constituting, alone or together with proliferating interstitial tissue, the round-celled deposit found in leukemic organs.

As regards indications for *treatment* derivable from these cases, may we not, with some show of reason argue, that, had the mother of these children been prevented from reducing her strength by over-suckling and other avoidable causes, her twin offspring would have been stronger than they were, if not so strong as those born singly? and that, had it been recognised that children of their type, and born under the circumstances in which they were born, were, notwithstanding their apparent health, prone to such a disease, may we not also contend that a watchful averting of all that was in any way likely to induce shock, or to debilitate, and an employment of all means calculated to maintain health at as high a standard as possible, would have postponed, if they could not altogether have averted, the extensive lymphadenoma which killed both?

In estimating the value of any line of treatment, even where apparent benefit ensues, as indicated by a fall in the temperature and pulse, and a diminution of the enlarged glands, we must ever remember that a *spontaneous* fluctuation in all these respects is liable to occur.

In the case of the young lady incidentally referred to, the following prescription, in conjunction with a generous diet, appeared temporarily beneficial:—℞ Quin. sulph. grs. xii.; tinct. nux vom. ℈i.; acid. phosph. dil. ʒi.; liq. secale cornut. ʒii.; aquæ ad. ℥vi.—Sig. A twelfth part to be taken every four hours, together with two pills, each containing three minims of spirit of turpentine.

The *rationale* of this somewhat elaborate recipe was, that the first three ingredients were calculated to reduce temperature and act as a nerve-stimulant, while the ergot and turpentine, it was hoped, might astringe the extreme capillaries in the parts affected, and prevent extravasation, which was regarded as the chief element in the actual enlargement of the glands. After having faithfully used these remedies for a week, the patient's condition improved in every respect, while a cervical gland, which had for a length of time before the commencement of this treatment remained as large as a hen's egg, was, at the end of the week, reduced to the size of a grape. The improvement, however, merely lasted a few days, though the same treatment was continued, and was not finally abandoned as of no permanent benefit till the end of the third week. Since then, two grains of quinine and ten minims of the tincture of the perchloride of iron, given three times a day, have proved as beneficial as the other prescription did at first. This improvement has continued for about a week; but the past conduct of the case forbids any confidence in predicting a persistence of this state of affairs.

The patient is a highly intelligent young woman, and says she has observed that, be the treatment what it may, the glands generally take about three months to enlarge and subside, a fresh enlargement always following the preceding subsidence.

Phosphorus, in minute doses, given interruptedly as a nerve-feeder, may be found useful; but its administration with the object of producing a fatty degeneration of the lymphadenomata seemed to be satisfactorily disposed of as not only useless, but absolutely injurious, by the evidence given, such as it was, at a recent discussion at the Clinical Society of London; as, unfortunately, vital organs appear to participate in the general disintegration.

I am not aware whether galvanization of the cerebro-spinal and sympathetic systems has been found of any service in cases of lymphadenoma; but, accepting for the sake of argument the possible correctness of the etiology of the disease set forth in this paper, one would expect greater results from galvanism used as above indicated as a vaso-motor tonic, together with the so-called strengthening remedies, including a nutritious diet, regulated according to the state of the patient's digestive capabilities, than from the milder deobstruants, or from phosphorus used as a degenerater.

## ARTICLE IV.—*Fæcal Accumulations, simulating Utero-Ovarian Tumours.* By Dr G. de GORREQUER GRIFFITH, Senior Physician to the Hospital for Diseases of Women and Children, Physician to St Saviour's Maternity.

(*Read before the Harveian Society of London*, 15*th February* 1877.)

IN the early days of my practice I had a female child brought to me with an enlargement in the left iliac fossa. I pronounced it to be a tumour, from its slow growth, its hard nodular condition, its only slight mobility, and from the entire history. I could not satisfy myself as to its nature, and asked one of my seniors in the profession to see the girl along with me. He thought also that it was a growing tumour; but he would not commit himself to a determinate diagnosis at this, his first, visit, and we agreed to watch the case together. The bowels, we had been assured, had been freely moved every day. Before our next visit the child had been sent away; and I learned that the enlargement had soon been reduced by means of aperients and enemata.

From that case I learned a valuable lesson, which has not been lost on subsequent patients.

The second case which I shall here quote is as follows:—In the autumn of 1876, I was hurriedly summoned to an old lady, who had, within a few days of my seeing her, met with a severe accident in the city, having been knocked down by a hansom as she was crossing the street. All her friends had given her up to die. She was so powerless to move, so prostrated, and so large a tumour, they stated to me, had made its appearance since her injuries. Her age (80) seemed to exclude all hope of recovery; and I was asked to see her—more that it should not be said she had died incapable of making her will, and to witness her signature to it, than with any idea that I could benefit her.

I examined the abdomen, and while doing so, learned from her that she thought she had been larger on the left side for some time before the accident. I found considerable enlargement of the entire abdomen from flatulent distension, and on the left side a tumour, hard and apparently irregular, extending from the left hypochondriac into the left iliac fossa, and passing a little way to the right of the mesian line. At first I thought it might be enlarged spleen, or a left ovarian dropsy, or an extra-uterine fibroid, which had been unnoticed, and was now observed, solely because attention was directed to the left side, where the patient had been struck by the vehicle. I could not at this, my first, visit, make a very minute examination, owing to the extreme prostration and depression; but at my second visit, having in the interval built her up and cheered her all I could, I examined very carefully per vaginam, and with equal care explored by the rectum. I then came to the conclusion that there were neither ovarian nor uterine tumours, and that I had to deal with an accumulation of fæces—even though the bowels were moved every day, as the attendant informed me, and that the accumulation had commenced previous to her accident, forming no doubt the enlargement which she told me she had noticed before her injury, and which, as the accumulation increased, culminated in the enlargement I found. I swept out the bowels by free purgation, kept up for some days, while I sustained her with light and easily digested nutrients, allowing as stimulant only good tea and coffee.

And here, in parenthesis, permit me to say, that in all ailments, when I can get a patient to follow my advice, I prefer as stimulants—non-medicinal—tea and coffee *made with boiling-milk*, instead of with water, a plan I invariably follow also with infants and children with most pronounced good effect.

To return to our case. The purging quite dissipated all enlargements, and the old lady's will has not yet made its way to Doctor's Commons. In this case, though I should have wished it, I was precluded from having injections given, because the patient objected, preferring to try the effect of aperients by the mouth; moreover, she would not have a trained nurse, and being a private patient, I could not compel her. Had the medicines given by the mouth been inoperative, then we should have tried coercion in the matter of the enemata; but as the aperients acted speedily, efficaciously, and in every way that could be desired, producing neither emesis, nausea, nor any other unpleasant effect, I followed the patient's inclination, and have no cause of regret as to the results.

The next case which I shall quote is that of Mrs G., aged 25, mother of three children, the last being about four months old when I was first in attendance. I was called up to her on the night of 18th June 1876, "as she was suffering acute pain in the left side, which she could endure no longer." On examining the abdomen I found a hard, irregular, exceedingly tender tumour, from which she was enduring great agony, and which was almost as large as an infant's head. I made no further examination that night, contenting myself with ordering her ½-grain morphia suppositories, to relieve not only the pain, but likewise the tenesmus and the passing of mucus. The discharge from the bowels was quite fluid, but distinctly fæcal, occasionally a scybalous mass making its appearance.

Next day, the morphia having taken good effect, I examined with the finger by the vagina, but could make out neither ovarian nor uterine tumour; the sound *in utero* enabled me to make certain that there was no intra-uterine growth; but movement of the uterus with the sound in the interior of it was attended with movement of the mass, which I found lay outside the womb, yet connected to the left and upper portion of it—in fact, attached to it. I gave it as my opinion that whatever the mass was, it was outside the uterus, and was adherent to it, and that it was not ovarian. I did not, however, express the opinion at which I arrived after the above examinations and after thoroughly exploring by the rectum, viz., that it was a case of impacted and accumulated fæces, which, having set up great irritation, had occasioned inflammation, effusion of lymph, and matting or glueing of the bowel to the left and upper portion or cornu of the uterus, that organ being still enlarged, its involution after delivery being not yet completed, probably owing to the irritation, inflammation, and subsequent adhesion to which I have referred. Taking this view of the case, I purged freely and continuously for some days, till at length after the lapse of six weeks I had the satisfaction of hearing from my patient—for I did not attend her continuously during this period—that the tumour was all gone, and she was quite well; facts I verified by careful manipulation when she last visited me. The iodide of potassium had been combined with the aperients, as had also anodynes,—the former in hope of dissolving adhesions, the latter with a view to ease pain. I would add, to show the difficulties which sometimes behedge the diagnosis in these cases, that this patient had previously had pronounced to her by three medical men that operation alone (gastrotomy) could do her any good; and of this she had a mortal dread, so that all through I buoyed her up with the hope that the knife might never be required.

The swelling had commenced to be noticed about twelve or fourteen days after the birth of her child, was chiefly confined to the left side, though sometimes it seemed to enlarge, and to extend higher up and across the middle line towards the right, and was so large that it was as though she was at her full time, and when walking, even across her room, she required a towel to support the abdomen; at other times it would subside, preserving, however, the same shape; these alterations in size were synchronous with the action of the bowels, and gave me a valuable clue. The agony had been very great, and she told me nothing had relieved her for any length of time till she had used the morphia suppositories. At no period was there a discharge of matter indicative of any internal abscess; nor any flux of water either into the abdominal cavity, or into the bladder, or any way externally, which would demonstrate the existence and rupture of an ovarian or other cystic growth; therefore, the only diagnosis at which I could arrive was that the bowels had become blocked during the confinement period, had not emptied themselves fully, that an accumulation occurred and became greater and greater, being however occasionally partially lessened by the aperient action of the bowels themselves, which accounted for the diminution and subsidence that had been noticed in the swelling.

I am the more encouraged to bring these cases forward, because you, Sir, our President (Mr Carr Jackson), and Mr Teevan, Dr Broadbent, and other speakers at our last meeting, advocated the free exploration of the bowel, even to the introduction of the hand and arm, after the fashion recommended by Professor Simon, of Heidelberg, and referred to, not unfavourably, by Spencer Wells, Dr Priestly, and others; and which I myself have advocated as the readiest method of assisting in the enucleation of intra-uterine growths, when ablation by that method is thought advisable. Dr Braxton Hicks has, I think, called attention to cases similar to those I record, and these all demonstrate clearly how very complete should be our examinations externally, per vaginam and per rectum, and the manner in which we should empty the bowels, when we are called to attend for the first time a patient suspected of having an intra-abdominal tumour.

I am not aware in what year Professor Simon first advocated and adopted the exploration of the interior of the abdomen by means of the hand and arm passed into the rectum; but I *am* fully aware that he laid great stress on the advantage of such a procedure as an aid to diagnosis. However, I cannot conclude my paper without pointing out that a most distinguished surgeon of our own city—Mr Maunder—adverted to the plan many years ago, and actually put it into practice in 1868, as he has informed me, and as we also learn by reading his remarks on "Diagnosis by Hand in the Rectum"—see *Lancet*, 1868, page 586, and vol. i., 469. In cases of lumbar colotomy he resorted to it—see *Medical Times*, 3d February 1877.

This eminent surgeon likewise practised the relief of constipation by galvanism—see *Medical Times*, 7th February 1863—a plan which, some years back, I tried myself to see if it would answer, and which, I am bound to say, had the result of stimulating the intestines to action. As regards the exploration of the bowels by means of the hand and arm, introduced per anum, it is hardly necessary to say that according to the size of the operator's hand and that of the patient's rectum there is a correlative forcible distension, and a corresponding rupture in the mucous membrane surrounding the anus, also of the sphincter ani; indeed, this latter has its fibres, in some cases, torn to such an extent that incontinence of fæces results—but for so short a time, the mischief being so quickly repaired, that a week or ten days suffices to set the parts right again. I have never known violent inflammation of the bowel to result, though I *have* seen inflamed piles follow the procedure as a direct sequence. Of course, if there were any suspicion of malignant disease affecting the bowel, the hand should not be forced through the anus. These are precautions which immediately suggest themselves, and hardly require to be mentioned; yet, as it is well to bear them in mind, I shall not withhold the warning.

In all cases of suspected fæcal accumulations, I advise warm-water, or warm-gruel enemata, or the injection of olive-oil, either mixed with the foregoing or by itself. In many instances these will be found sufficient without the employment of any aperients by the mouth.

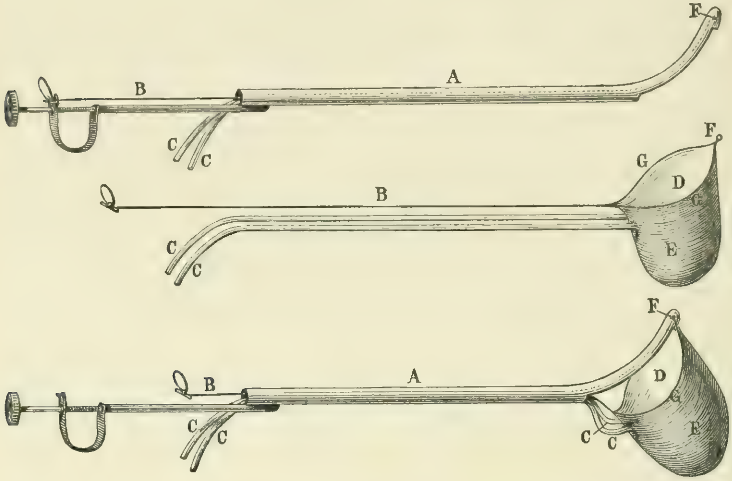
In the last case which I have quoted, though I advised enemata of warm water, hoping they would be found to have a soothing and emollient effect, the patient had not the advice executed, as she feared the pains in the tumour (?) and in the bowels would be thereby increased.

## ARTICLE V.—*Litholysis.* By GEORGE C. DUNCAN, M.D.

IN fulfilment of the promise given in the introductory notice of this subject in the October number of the *Edinburgh Medical Journal*, I will now attempt to lay before your readers a more minute description of the instrument; but, before doing so, I wish it to be distinctly understood that I do not pretend to say that all urinary calculi can be got rid of by litholysis, but that some cases may be successfully operated upon; and, if the percentage of these be ever so small, it is, I think, worthy to be placed in the list of means employed for their removal.

In my former article I stated that the method proposed was to encase the calculus in a thin pouch of indiarubber, introduced by a suitable contrivance, which is somewhat similar to an ordinary catheter, the difference being that about one-third of the convex side of the curved part is wanting, making a sort of trough in which lies the rubber pouch when empty, and in readiness for either insertion or extraction; and entering this pouch are two rubber tubes which communicate from it through the catheter to the operator.

An examination of the annexed diagram will probably help to give a better idea.



The instrument consists, in the first place, of an ordinary catheter, (A) gold-plated to prevent any corrosive action taking place from any of the acids used coming in contact with it. Running through this catheter is a small stiff steel wire (B) and attached to one end of an elliptical steel spring (G), which is fixed at the other end to the point of the catheter, where it works on the hinged joint (F), so that when the wire (B) is forced through the catheter, the spring which is fixed at the joint (F) is allowed to open, and, by drawing the wire (B) in the opposite direction, the spring is forced to close and lie in the trough of the catheter. Attached to this spring (G) is a rubber pouch (E), the mouth (D) of which is surrounded by the spring (G), so that the opening and closing of the pouch is accomplished by the pushing and drawing of the wire (B). This pouch is made of the thinnest rubber very highly vulcanized, ensuring it of elasticity and strength, yet taking up no more room than the trough of the catheter can easily afford. The size of this pouch is about two inches in depth, and the diameter about the same size as the spring, which is about two and a half inches in its long diameter, and one and three-quarters in its short, when open to its full capacity. The wire attached to this spring (which in other words is the mouth of the pouch), when withdrawn, is securely fastened to a contrivance of which no other description need be given than that it is arranged with a screw by which any amount of traction can be applied to the wire

(B), which ensures a safe closure of the pouch (E), as the curvature of the catheter serves, as it were, for its *point d'appui.* Entering this pouch at the side next the operator are two small rubber tubes (CC), their entrance being close at the edge, just allowing free communication with the interior when it is closed; these, as before stated, pass through the interior of the catheter to the operator. As will be seen by the above, the mouth of the pouch must be closed, and the bulk of it placed in the hollow of the catheter before introducing it into the bladder; if, then, by using the catheter as a sound, the situation of the stone can easily be ascertained, which, being done, pass the instrument to one side, so that the stone will be on the other side of the instrument to that on which lies the pendant part of the pouch, or, in other words, in apposition to the mouth of the pouch; then, by forcing the wire or stilet through the catheter, this mouth is opened ready for the entrance of the stone, which can be accomplished by making a lateral movement with the outer end of the catheter. It can be ascertained when in the entrance by drawing on the wire, which, of course, cannot be withdrawn, if there, to its usual distance. Having arrived at this stage, it will be found to be the safest plan to firmly hold the stone in the entrance until, by pressing the catheter to one side, the mouth of the pouch is nearly in a horizontal position; then, by withdrawing the traction from the wire, the spring opens of its own accord and allows the stone to drop into the pouch, which, being accomplished, the wire is drawn out and fastened to the apparatus, by which a sufficient amount of traction can be given to ensure a complete closure of the entrance of the pouch. The stone, being secured in the pouch, perfectly isolates it from the walls of the bladder, so that the next step in the operation is the introduction of the solvent, which is done by a graduated glass syringe, the nozzle of which fits into the end of one of the tubes. It may be as well to mention here, that the object for the syringe being graduated is to see that the same amount of fluid escapes from the exit tube that is forced into the pouch, for fear that some of the solution might escape into the bladder owing to some imperfection in the closure of the entrance.

In theory, different solvents will be required for the solution of the different formations; but, practically, nitric acid will, I think, be the solvent; I might say, *par excellos*, even the cholesterine formation is said to be soluble in it as well as in alcohol. No doubt, it will be at once asked, Will not the acid destroy the pouch and tubes, and in what strength will it be used?

Experience has taught me that the rubber is not acted upon by the acid, and the strength will be according to the length of time wished for the operation, and the size of the stone, with the general state of the patient. A solution containing about fifty per cent of acid disintegrates them very rapidly. The operator must be governed in the strength of his solution chiefly by the formation, of which the microscope must be his guide. I will have to defer until another time some experiments concerning the different degrees of solubility of the different formation of vesical calculi, which I wished to have given here, on account of having been disappointed in the receipt of some specimens promised. It may also naturally be asked, in case of any rupture or imperfect closure of the pouch, what will then be the consequence, and what must be done? By having a graduated gauge on both the tube of entrance and exit, it can easily be ascertained when the same quantity does not escape that has been injected, and if such an accident should happen, by having taken the precaution before introducing the instrument to have the bladder partly filled with urine, so that the small amount of acid that might escape will be diluted to such a degree that no harm to the bladder will result. As a precaution, it might be well to have an extra syringe and an alkaline solution at hand, so that it could be injected through the catheter into the bladder, thus thoroughly preventing any farther trouble. My reason for mentioning this is, that an objection has been frequently made to me while discussing the feasibility of the plan, which was, that the formation of gas from the disintegrating stone might, if the tubes became obstructed, burst the pouch and allow the acid to escape into the bladder. Allowing that such an accident should occur, and taking into consideration the small amount of acid in the pouch, and the neutralizing effect of the stone, together with the dilution it would receive from the urine, and the injection of the fluid from without, I do not think that any serious harm would result.

Electricity has been proposed to disintegrate stone; but I have had no experience with it as yet. It has occurred to me, if it is of use, that a circuit could be made by passing wires through the tubes instead of fluid, so that the stone would be between the poles of the battery.

## ARTICLE VI.—*Note of a Case of Painless Second Stage of Labour in a Primipara.* By Dr C. E. UNDERHILL.

(*Read before the Obstetrical Society of Edinburgh, 14th February* 1877.)

M. H., aged 16 years and a few days; unmarried, primipara; a poor, small, ill-developed girl, physically and mentally, menstruated last in the middle of September 1874, and fell in labour on 15th June 1875. I was called to her at 3 A.M., 16th June. The os was then about half dilated, membranes entire, head presenting in first position of Naegelè, pains frequent, occurring about every seven or eight minutes, and severe. She bore them badly, crying and uttering great expressions of suffering, as each pain came on. This continued until about 6 A.M., when the waters broke, and I was shortly afterwards struck by the fact, that she gave no more indications of suffering, and indeed kept quite quiet. Thinking that the labour pains had left her, I asked if she had no pain, and she said, None. While standing by the bed, I noticed the drawing up together of the body, and the holding of the breath, characteristic of the expulsive pains of the second stage of labour; and with a hand on the uterus, I felt active contraction going on, followed shortly by relaxation. This occurred again and again, and each time in answer to my question, she declared that she felt no pain. Meanwhile the head was slowly advancing and distending the vagina and perineum, the pains succeeding one another with the ordinary rapidity and duration of a tolerably severe first labour; still the patient stated that she did not feel any pain, and I am convinced that she did not, except just as the head was being born, when she uttered some exclamations of distress. She did not appear to suffer at all from the time when the waters broke, and the second stage of labour began, until the birth of the child,—a period of nearly three hours. The child was a living female of average size; the fourchette was untorn. I was called away shortly after it was born, and was told by the friends that soon after I left the patient had a fainting fit. She was quite conscious and wide awake throughout the latter part of the labour. She made a good recovery.

*Remarks.*—The circumstance in this case to which I wish to draw attention is the complete absence of pain during the expulsive stage of labour, lasting more than two hours, in a person who had complained of great suffering during the earlier part of the process, and who was naturally of a complaining character and deficient in self-control. That the whole process of labour may take place with little or no pain, even in primipara, is well known; but these cases are generally rapid and easy. Indeed, Dr Montgomery relates an instance in which a young primiparous woman was delivered by himself while she was sleeping naturally and soundly. He says:—"In March 1848 I attended a lady between nineteen and twenty years of age in her first confinement; her pains, which lasted altogether about six hours, were all through sharp and trying; as the labour drew towards its close she suffered less distress, and slept soundly at intervals between the expulsive efforts, as is very usual at such a time; but about five minutes before the birth of her child she fell into a deep sleep, and did not waken until after I had divided the funis. . . I was standing by her all the time, and can therefore state with certainty that she took no anodyne or anæsthetic of any kind."[1]

He looks upon this as a remarkable case.

Here, as in my case, the pains in the earlier part of the labour were severe, while the period of comparative immunity was apparently only at the very end of the process. The same authority quotes cases in which pregnant women, who had previously borne several children, were awakened out of sleep to find to their astonishment that their children had been born without their being conscious of the occurrence of the labour. In another paper he says:—"I have met with but one instance of a woman who bore children without pain. That lady had eight children, and she never gave birth to one of them without being in the most imminent danger of losing her life from hæmorrhage."[1] In all these cases the labours are stated to have been very quick. Dr Protheroe Smith relates a case which he once witnessed of painless labour unaided by artificial means. The subject of it was a young lady, a primipara; and during the dilatation of the os, the passage of the child through the pelvis and soft parts, the distension of the perineum, and the final expulsion of the contents of the uterus, there was not only no complaint, but in answer to his repeated inquiry whether she did not suffer, she as often assured him that she felt no pain. The patient was in good health at the time, nor could any peculiarity of constitution or condition be detected, which would satisfactorily account for her immunity from the ordinary sorrows of childbirth. A similar case had been related to him by Mr H. Callaway.[2]

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[1] Signs and Symptoms of Pregnancy, 2d edition, 1863, p. 608.

I do not think such cases are common, and I bring this one forward merely for the purpose of recording it. I can offer no explanation of this freedom from pain during a long and tedious second stage.

## ARTICLE VII.—*Case of Empyema* (under the care of Mr JOSEPH BELL, F.R.C.S. Edin.). Reported by ROBERT KIRK, M.B., L.R.C.S.

THE following case of empyema, resulting from a severe blow on the upper dorsal vertebræ, is interesting on account of the rapidity of cure which followed the introduction of thorough drainage with antiseptic precautions:—

A. W., aged 46, a bleacher, was admitted into Ward VI. on 28th October 1876, suffering from difficulty of breathing and a fœtid discharge from a sinus on the left side.

The history which he gave of his ailment was, that eighteen months before his admission patient fell from a tree, a height of twenty-five feet, alighting on his shoulders and upper dorsal vertebræ. He lay unconscious for eight weeks at his home, and on recovering consciousness found that the power of his limbs was much impaired. A fortnight later he went to a provincial hospital, and was admitted as an in-patient. It was there discovered that an abscess existed under his left axilla, pointing between the fifth and sixth ribs. Poultices were applied for a fortnight. The abscess was then opened, and a large quantity of pus was evacuated. Simple water-dressing was employed, and putrefaction ensued, the patient leaving hospital after being under treatment for ten months, the discharge being very profuse.

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[1] Dublin Medical Journal, 1849, p. 332.

[2] Scriptural Authority for the Mitigation of the Pains of Labour by Chloroform. By Protheroe Smith, M.D. London, 1848, p. 33.

For about six months he remained at home, gradually losing ground, the discharge being very fœtid, and so profuse as soon to reduce him to a state of great emaciation.

On admission his expression was anxious, he looked old, and he was much emaciated. On inspection his chest was seen to be flattened on the left side, the apex beat was noticed almost in the middle line, and the respiratory movements were confined to the right side. The lung sounds on the right side were normal, although the breathing was hurried; those on the left were scarcely audible. Percussion elicited very extensive dulness over the left side extending posteriorly almost to the apex of the left lung. A bullet-probe was passed into the sinus by Mr Bell as far as the bodies of the dorsal vertebræ, one of which was bare and rough. The sinus was injected with 1–40 carbolic solution, and the ordinary antiseptic gauze dressings were applied. The discharge continuing to be profuse and fœtid, a silver perforated tube, with a circular shield, to prevent it slipping into the chest, and with a curve corresponding to that of the ribs, was introduced into the sinus, with the result of allowing of the escape of eighty ounces of horribly putrid pus. The cavity was then washed out with the carbolic solution under the spray, and was carefully emptied by the patient making attempts to draw in a full breath. At the evening visit the dressings were changed, the cavity again washed out, and a large quantity of pus evacuted. Next day the discharge was less, about fifty ounces, and was less putrid; and this improvement went on, the discharge continuing to decrease and the putridity to diminish until 18th November, when a few drops of serum only were excreted. The wound was dressed every two days; the discharge was quite sweet, and the injections of carbolic lotion were persevered with until 28th November, when after the usual injection the patient's pulse fell from 72 to 40, his lips paled slightly, and he looked as if he would die of syncope. How this action was produced is rather doubtful, but the probability is that it was from irritation of the vagus. Next day a warm solution of carbolic acid was employed for injection, but as a return of these symptoms occurred, although to a much less degree, the use of injections was discontinued. This was fortunately followed by no increase of discharge, the protective being only moistened with serum at each dressing, and the sinus gradually shortened, as was indicated by pain of a cutting character, which was at once removed by shortening the tube. This was done on three different occasions, and on 9th February it was removed altogether. The lung at this time was quite in contact with the wall of the chest, the heart had gone back to its normal position, and the dulness on the left side had disappeared. Perhaps the most satisfactory proof of his improvement in general health is that during the last month he was in hospital he gained ten pounds in weight. On 22d February he left for the Convalescent Home, and while there improved much in his general health, being able to walk to hospital, a distance of three miles, twice a week to be dressed. On 8th March he left perfectly cured.

This result, no doubt, was due to the efficient drainage kept up by the silver tube, and also to the antiseptic dressings. At one time it was feared that carbolic poisoning with its characteristic action on the urine would supervene on the introduction of so large a quantity of carbolic acid into the abscess cavity, but fortunately this untoward result did not take place.

## ARTICLE VIII.—*Note on the Use of the Stethoscope in Obstetrics.* By JAMES CUMMING, M.D., F.R.C.P., Edin., Extra Physician to the Royal Hospital for Sick Children; Physician to the New Town Dispensary, Edinburgh.

(*Read before the Obstetrical Society of Edinburgh*, 28*th March* 1877.)

MRS K., æt. 41, Bonnington, consulted me last year at the New Town Dispensary for a mitral systolic murmur of the heart. She had suffered several years ago from a severe attack of rheumatism. On the 18th of last October (1876) she again consulted me at the Dispensary, stating she was afraid she was suffering from an abdominal tumour which had been steadily increasing in dimensions during the past few weeks. I suggested pregnancy, but she denied the possibility of this, having experienced none of those symptoms which had accompanied all her previous pregnancies (morning sickness, fœtal movements usually felt about the seventeenth week, etc.), and gave me the following brief history:—Has had six children. At her last confinement, which occurred in March 1868, was attended by a midwife, when she had two severe floodings. Was last unwell towards the end of April (1876). I thought, before proceeding to make a thorough examination, we might see what information could be gained from the use of the stethoscope. Accordingly, having applied it to the abdomen, between the umbilicus and the right anterior superior spine of the ilium, I heard a loud blowing bruit, which, however, was not confined to this spot, but was audible all over the abdomen. On the patient assuming the prone posture, this sound was not in the slightest degree diminished.

With regard to the uterine souffle I may mention here in parenthesis, that on glancing over one of the French medical journals recently,[1] I observed that at a meeting of the Académie de Médecine, held on the 6th of last June (1876), M. Bouillaud still

[1] Archives générales de médecine.

adhered to the *abdominal* theory regarding the cause of the souffle, promulgated by him in the year 1835,[1] viz., that it is due to pressure of the gravid uterus on the large arterial vessels situated posteriorly (aorta and iliac arteries). The younger Glénard of Lyons, on the other hand, advocated the théorie épigastrique (that it is caused by pressure on the epigastric artery).

At a meeting of this scientific society, held on the 27th of the same month, M. Depaul replied to the above statements, mentioning that the theory of Glénard is by no means a new one, as it exists in a work published by Kiwisch in 1849. However, in a communication addressed to Depaul, Glénard states that he believes the true seat of the souffle to be in a neighbouring artery of the epigastric, an anastomosing branch between the internal iliac and the ovarian, which he proposes to call the puerperal artery.

Let us consider for a moment the arterial circulation here. The epigastric artery, a small branch of the external iliac given off a few lines above Poupart's ligament, ascends upwards and inwards between the peritoneum and the transversalis fascia, enters the sheath of the rectus abdominis, supplies that muscle, and anastomoses with the superior branch of the internal mammary and inferior intercostal arteries. The ovarian, tiny vessels arising from the anterior part of the aorta, a little below the renal arteries, are distributed to the ovaries and uterus. Would pressure on those vessels account for the uterine souffle? It seems to us that no comments are required on this untenable theory. Depaul, although not denying the possibility of a bruit in the epigastric artery, argues that it cannot be compared to the bruit de souffle, which is due to the arteries ramifying in the walls of the uterus, so much developed during pregnancy. Here are his own words:—"Ni par la théorie du souffle épigastrique, ni par celle de M. Bouillaud qui le place dans les grosses artères iliaques, on ne peut expliquer comment ce bruit peut être entendu dès la fin du 3$^e$ mois de la grossesse alors que l'utérus n'a pas encore dépassé le détroit supérieur et ne peut, par conséquent, comprimer ni l'artère épigastrique, ni l'iliaque interne. Du reste, ce bruit de souffle ne s'entend pas seulement chez les femmes enceintes, mais plusieurs heures et même plusieurs jours après l'accouchement." Rapin of Lausanne also supports the uterine theory. This is the explanation generally accepted, and hence we name it the *uterine* souffle.

To continue my case. Having heard what is only a symptom, I resolved to search for the only true sign of pregnancy, the fœtal heart. On planting the stethoscope in the mesial line of the abdomen, about an inch below the umbilicus, and using some pressure, a pulsation was heard loud and distinct, beating at 130 per minute, while the patient's pulse was under 90. I also thought I detected movements of the fœtus. One of my pupils also made an auscultatory examination and corroborated me, so we were able, much to her relief, to assure her that the tumour was that of pregnancy. This turned out to be about the fifth and a half month of utero-gestation.

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[1] Traité clinique de maladies du cœur.

Now, we know that the fœtal heart is audible at a considerably earlier period than this,[1] and I feel confident that, had we had the opportunity of making an examination, it would have proved so in this case, judging from the character of the pulsations (loud, distinct, and readily detected).

She asked me to attend her at her confinement, which I consented to do, thinking it might turn out to be a case of some interest.

On the 30th of last January, at 1 A.M., I received a message to see her, and on examination found the os uteri about the size of half a crown. She had been in labour some hours, but the pains were not strong. At 2 A.M., finding that but little progress had been made, and as she felt inclined to sleep, I left her.

On returning at 9·30 A.M. the os was fully dilated, but the head high up in the pelvis. She complained of being much exhausted, and although the pains were pretty frequent, they were short and unsatisfactory; and at 10.30, finding no advance of the head, I resolved to apply the long forceps.

The bowels and bladder having been shortly before relieved, and the position of the head (a first) ascertained, with the assistance of a neighbour who acted as nurse, I placed the patient in the usual position, on her left side, administered chloroform (with considerable reluctance on my part, and only at her urgent request), and proceeded to apply the instruments. It is recommended in text-books to introduce two or three fingers of one hand, but in this case it was simply impossible from the extreme rigidity of the parts; so, trusting mainly to the forefinger of my left hand as a guide, I introduced the lower blade in a direction corresponding to the left oblique diameter of the pelvis, taking care to keep it close to the child's head. Then feeling with my right forefinger, holding the upper blade in my left hand, I introduced it in front of the lower one, and raising it slowly and gently, it glided over the right side of the head, when the blades readily locked. The forceps were thus fixed in a direction corresponding to the left sacro-iliac synchondrosis and the right foramen ovale. I much felt the want of skilled assistance. Considerable traction in the axes of the passages was required, and when the head was born the umbilical cord was found to be tightly bound round the neck. This was at once freed, but remembering the old rule, not to cause a sudden evacuation of the uterus, more especially if from the previous history of the case we have reason to apprehend flooding, I merely placed my hand on the fundus uteri, and, exerting a moderate amount of pressure, followed the descent of the child till it was born a few minutes later. It was a male, apparently of average weight.[1] When the patient came out of the chloroform (which she did almost immediately after the birth) and was told that her child was alive and well, she calmly informed me that she was going to flood. The uterus was contracting fairly under friction; but thinking the application of cold might hasten the process, I went to the window-sill and taking a handful of snow, added it to a basin half filled with cold water, and, dipping my hands for a few seconds in this freezing mixture, applied one over the uterus externally, and with the other introduced internally, removed a few small clots. The patient gave a gasp. The uterus soon contracted and felt hard and firm under the hand, like a cricket-ball. She made a rapid and excellent recovery.

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[1] Edinburgh Medical Journal, October 1876, p. 322.

# Part Second.

## REVIEWS.

*Hints for Hospital Nurses.* Arranged by Rachel Williams, of St Mary's Hospital, London, late of the Edinburgh Royal Infirmary; and Alice Fisher, of the Fever Hospital, Newcastle-on-Tyne. Edinburgh: Maclachlan and Stewart: 1877. Pp. 174.

This little book indicates much care in its preparation, and is a very satisfactory testimony, at once to the attainments of its accomplished authors, and to the superior style of teaching which the rising generation of nurses receive. The reader who can look back, were it only for ten or fifteen years, must confess that a great change for the better is now in progress, and that, while excellent specimens of what an hospital nurse ought to be, may rise up to his memory during the old *regime*, still there were many shortcomings and abuses in those days, which are very unlikely to be met with now.

We cannot help regarding the present zeal which women exhibit for improving the whole arrangements regarding the education and training of their own sex as nurses, and the cordial self-devotion with which many educated women consecrate their lives to that service, as the providential and felicitous outcome of the movement, so perplexing to the medical schools a few years ago, when lady students of medicine were pressing strenuously for access to our halls and colleges. Some of their advocates at that time may imagine that they have now, in great measure, gained their object; but, if we mistake not, the fair aspirants who will henceforward desire a full medical education are comparatively few. The vessel has entered the desired haven; but her sails are flapping, and as the cause of this sudden change we regard the reasonable and successful inauguration of a new undertaking—the training of a superior class of nurses.

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[1] Edinburgh Medical Journal, November 1875, page 421.

We are specially pleased with the three introductory chapters. The advice therein tendered to probationers is sensible, judicious, and high-toned, and deserves to be carefully considered by all who are tempted to offer their services.

"First of all, I would most earnestly entreat no woman to allow love of notoriety, false sentiment, or even a mere honest wish of earning her own livelihood, to influence her in her choice. I am sure, that of all the means by which it is possible to gain daily bread, there is none more irksome, more trying, nay, more positively revolting, than the charge of the sick, if the nurse have not that inborn love of the work which is given, alas! but to few. But, on the other hand, if it happens that she have this love in addition to other necessary mental and physical qualities, no calling can be found which offers so happy a life, or where labour brings so quick or inevitable a reward, or which, in spite of many undeniable anxieties, secures such absolute peace of mind."

"Do everything not only without grumbling, but with ready pleasure. The value of sunshine in hospital work can hardly be overestimated. Keep the peace with everybody, which you will find you can only manage by extreme moderation in the use of your tongue, and do everything, down to the smallest, most menial office, as in the immediate presence of the Great Master, as though the Lord, indeed, were at hand."

The more didactic portion of the work exhibits, we think, much judgment, and shows how well the writers have digested the instruction given by their teachers. All the salient points in the management of cases ordinarily treated by the physician are attended to and emphasized; and the hints for surgical nurses both in the wards and in the operating theatre are given with intelligence, precision, and conciseness. The antiseptic treatment of wounds, ulcers, and sores, is admirably expounded, from the nurses' standpoint, both as to principle and minute details.

We like the following. It indicates good sense and good feeling:—

"The surgical dressings in a ward are sometimes done by a nurse, and sometimes by a dresser; and I am sorry to say there is every now and then a little uncomfortable feeling between them as to who has the best right to the privilege of doing so; for as both dresser and nurse are students, a privilege it must always be considered. But as the former pays a great deal more highly for it than the other, I must say I think he is more entitled to it, and a nurse should learn to consider it as a favour and not as a right when she has dressings at all confided to her."

We cordially recommend this little book to all who wish to be useful to the sick, to mothers of families, to ladies looking forward to Zenana work in India, no less than to nurses and Bible-women at home.

*Nutrition in Health and Disease: A Contribution to Hygiene and to Clinical Medicine.* By JAMES HENRY BENNET, M.D., Member of the Royal College of Physicians, London, etc., etc. Second Edition. London: J. and A. Churchill: 1876.

WE have read this book with satisfaction, and can cordially recommend it as a good digest, brought down to the present day, of the received facts and doctrines of physiology and their bearings on the preservation of health and the arrest of morbid processes. It is not the work of a stripling ambitious of notoriety and desirous of professional employment. It expresses the mature opinions of a physician who has written very creditable books during the last thirty years, and having been compelled by the state of his own health to spend his winters at Mentone for a lengthened period, has last year retired entirely from practice in England, and has employed his first summer's leisure in revising former scientific researches, and reappearing once more in the field of hygienic and sanitary science. This sacrifice of what might have been a pleasant and luxurious holiday among the sunny fields and under the trees of his country home, indicates the writer's own conviction that he has something to say worthy of being heard, and ought to secure for his book a careful and friendly perusal. Our space forbids more than a passing notice; but we would draw special attention to the importance which Dr Bennet attaches to the simple ocular inspection by dyspeptics of their own urine a few hours after taking food. "If it becomes turbid, the thermometer being under 50° or 60°, they may be certain that they have not perfectly digested the previous meal; that there is 'something wrong;' that there is a cloud on the horizon. Either they are getting out of health, and the digestive functions are depraved, or there has been too much exertion of mind or body, or the meal has been an error as to quantity, quality, or time. In the one case, the alarm should be taken, and the health attended to; in the other, the error should be ascertained and avoided." Are we not too apt, as Dr Bennet suggests, to regard certain organic diseases of the liver and kidneys as *distinct entities,* instead of viewing them as the results of the constant labour which these organs undergo in eliminating the *food-poison* occasioned by dietetic and other errors of hygiene? Constitutional and hereditary taints are brought to light and developed, and chronic inflammations are set up, thus explaining the varied forms of cirrhosis in the liver, of albuminuria in the kidneys, of diabetes in the chylopoietic viscera generally. The subject of dietetics in health and disease is treated with ability; and many wise things are said upon the indiscriminate prescription of stimulant beverages. There is a serious error, we believe, in having recourse to these in many cases of defective assimilation. The benefit, when it does occur, is temporary, while both physician and patient are deceived as to the real dangers and necessities of the case. We cordially recommend the book as an important contribution to clinical medicine.

*The Ancient Life-History of the Earth: a Comprehensive Outline of the Principles and Leading Facts of Palæontological Science.* By H. ALLEYNE NICHOLSON, M.D., etc., Professor of Natural History in the University of St Andrews. Edinburgh and London: W. Blackwood and Sons: 1877.

THE appearance of the volume before us must have excited some degree of surprise amongst various groups of readers, and most of all amongst those who are acquainted with Dr Nicholson's work on "Palæontology," published some five years ago. The present manual appears to consist of an extension of the section devoted to "Historical Palæontology" in the previous volume; and according to the most natural of developmental laws—operating within a limited period after the origin of the former work—as might be expected, a very close similarity exists between the two books. The woodcuts which appeared in the Palæontology, supplemented by several new figures, are used in the illustration of the work before us; and in the descriptions of the various extinct fauna, the author in many cases repeats much of his former writing. The work is, as we learn from the preface, principally intended for the use of geological students; the author, however, stating "that the method of treatment adopted has been so far untechnical as not to render the work useless to the general reader who may desire to acquire some knowledge of a subject of such vast and universal interest." We confess to being rather sceptical of the interest of that anomalous person, the "general reader," in a work of this kind. A reader, to take any interest worthy the name in a work which bristles page by page with scientific terms, must, in our opinion, possess aims which cannot, by any stretch of the imagination, be named "general," but which must rather be accounted of the most special kind. Hence we may again express the opinion that with the author's "Palæontology" still in the market, the advent of the present work might afford a text whereon a literary Malthus might aptly inveigh against over-fertility in the department of ancient life-science. The only portion of the "Ancient Life-History of the Earth" which claims special attention from the reviewer as presenting him with, in a sense, new material, is that comprised within the first sixty-one pages of the manual. These pages are devoted to a short *resumé* of the "Principles of Palæontology;" and whilst they contain nothing novel or very original, present a readable account of the relations of geology proper to the geology and botany of the past. The doctrine of "Catastrophism" is well placed in contrast to "Uniformitarianism;" but the *quæstio vexata* of geological time is briefly dismissed, without special and required reference being made to its relativity. In the author's descriptions of the process of fossilization we meet with a repetition of the ideas which have been ventilated over and over again in geological text-books. Even the simile given in page 14, whereby a form of fossilization is compared to the rebuilding of a house brick by brick, is by no means novel; the process being similarly described in Jukes' "Students' Manual of Geology" (third edition, p. 477). Chapter II. is devoted to the consideration of the "Fossiliferous Rocks." Here, again, Dr Nicholson is simply reiterating information to be found in every text-book of geology pure and simple; and if, as he states in the preface, he has been forced to make the work "very limited in its scope," we might suggest that the space devoted to many of the preliminary geological details could have been aptly occupied by other and more appropriate matter. Only, as Dr Nicholson was bound to vary the present work as much as possible from his "Palæontology," the omission of these details would have had the effect of engendering a yet closer resemblance than actually exists between his—we had almost called them twin—literary offspring. If any evidence were wanting regarding the identity of the Palæontology and the present manual, such would be found when the reader, who had perused the former work, arrived at Chapter V. of the present volume. This chapter (V.) consists simply of the similarly numbered chapter from the "Palæontology," which has been bodily transported therefrom to do duty in the present instance, and which is given word for word in the "Ancient Life-History of the Earth," with a new concluding paragraph of some twenty lines or so. This process of bookmaking is hardly to be compared with that of perfect fossilization, inasmuch as the particles in the latter case are replaced each by a particle of different kind. In Dr Nicholson's work the particles of the new work represent in many cases simply the same old particles without a touch of the "metamorphism."

The great bulk of the book, devoted to a review of the fauna of the rocks as arranged in chronological or historical order, may be described as a simple manual of Palæontology, through which purely geological facts are copiously scattered. Throughout the entire range of the work we have been unable to distinguish any features differing from those already familiar to readers of geological and palæontological works; and a cursory glance at the details presented in various parts of the volume shows that the author must have had paste-pot and scissors, and an unbound copy of his "Palæontology," in tolerably frequent requisition,—*e.g.*, the description of Labyrinthodon (p. 215 of the present work), or that of Plesiosaurus (p. 244), or Pterodactyle (p. 245), whilst the description of the Mammoth (pp. 358, 359) is simply derived in greater part from a cutting taken from the author's former work. We must enter a strong protest against this method of making books, especially when readers are led to believe that an entirely new book has been brought under their notice. There is not, as far as we can ascertain, the slightest acknowledgment in this work of its indebtedness to the author's former treatise; and we entertain very decided views as to the duty of an author to clearly state to the public, in such a case as the present, that his book is a *rechauffée* of a previous work. A reader possessing the Palæontology, and who purchases Dr Nicholson's present work for a new volume, will not, we fancy, be pleased or gratified to find that he already possesses the work in embryo-form.

The concluding chapter of the work deals with "The Succession of Life upon the Globe," Dr Nicholson appearing as a supporter of "some orderly and constantly acting law of modification and evolution," although what that law may be according to the author we are not told. Evolution, as supported and as believed in by other observers, does not appear to contain any phase in strict agreement with the author's notions; and we must demur to the unqualified assertion, contained in the last sentence of his work, namely, that to palæontology alone we must look for the solution of the problem dealing with the origin of living beings. Palæontological evidence is of the most valuable kind, but that admission does not imply that it forms the only kind of evidence to be relied upon. The extent and limits of variation in living species, for example, form a subject of as vital import to the support or denial of evolution as the generalizations of palæontology.

The work, it should be mentioned, is copiously illustrated; the illustrations derived from D'Orbigny's "Cours Elémentaire de Paléontologie," used, as already mentioned, in the author's former treatise, being again employed in the present instance. Some of the "original" woodcuts (*e.g.*, figs. 9, 11, 15, and 16) strike us as being rather roughly executed, and as failing to convey an adequate idea of the beauty and delicacy of microscopic organisms especially.

We cannot lay down Dr Nicholson's work without expressing regret that time and labour have been thrown away on the republication of details which are not likely to appeal to general readers from their technical nature. The present work, being a kind of hybrid production, cannot be ranked either with pure geological text-books on the one hand, or with those dealing with palæontology proper on the other. Whilst considering the existence of important and reliable works on each of these departments, we are unable to see that the "Ancient Life History of the Earth" will supply any vacant place, or occupy any prominent niche in our scientific literature.—A. W.

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*Traité de Thérapeutique et de Matière Médicale.* Par TROUSSEAU et PIDOUX. *Neuvième édition, revue et augmentée avec la collaboration de* Constantin Paul. Deux tomes. Pp. 971 et pp. 1265. P. Asselin: Paris, 1875.

THIS is the ninth edition of a justly appreciated treatise on therapeutics, containing a practical digest of information regarding medicines, their properties, and their uses.

Any work of this description, so comprehensive in its plan, and necessarily deriving its materials from many discordant sources, will be subject, in some of its details, to adverse criticism. Physicians who compare its statements with their own personal experience and general knowledge of the particular medicines with which they are most familiar, are pretty certain to find statements in the book, which, according to their judgment, require modification, amplification, correction, abridgment, or suppression. Moreover, irrespective of individual predilections for particular medicines, and particular modes of employing them, it must be remembered that much of the discordance of opinion as to the properties and the doses of articles of the materia medica arises from the cases in which they are stated to have been useful or useless, not admitting of comparison from their being either essentially different or from their being loosely described. Shortcomings in therapeutic observations exceedingly increase the difficulties with which authors of systematic treatises on therapeutics have to contend. They are obliged to attempt an impossibility—they are forced, after a fashion, to generalize from facts which are, or may be, essentially dissimilar.

Having said thus much on the general question of fault-finding, we select an article for criticism.

In the work before us, we read, at p. 179, "*Creasote is a new medicine!*"—a strange statement to make in 1877, seeing that Christison, in the second edition of his Dispensatory, published at Edinburgh in 1848—twenty-nine years ago—wrote as follows:—

"In 1830, Reichenbach discovered, in the products of the destructive distillation of wood, a variety of singular compounds of hydrogen, carbon, and oxygen, to which he gave the names of paraffin, eupion, pittacal picamar, capnomor, and *creasote.* The last of them has alone been put to any use; and *has quickly become an important article of the Materia Medica.*" It is surely a mistake to say in Paris in 1877—"La Créasote est un médicament nouveau," Christison having described creasote as "an important article" in 1848, giving at the same time an excellent summary of its physiological properties, therapeutical applications, and pharmaceutical requirements.

The following extract discloses the number of years which elapsed after creasote was appreciated in Edinburgh before it was much discussed in Paris as a therapeutic agent; and, likewise, by giving us a precise date, affords a very probable explanation of the current French estimate of it as an internal medicine :—

"As soon as it was introduced into practice, a great emulation was excited among therapeutists as to who should discover new virtues in the new remedy. Cancer, dartrous affections, caries of the bones, scrofula, and phthisis were announced to have been cured by creasote. Under this escort, about the year 1859, creasote was introduced in France. For some months there prevailed a sad and deplorable infatuation [*engouement*], during which period the Institute and the Academy of Medicine were besieged by memoirs. The principal works addressed to the Academy of Medicine were those of Caster, d'Yvan, and d'Huc. They formed the subject of a very impartial report [*Mémoires de l'Académie Royale de Médecine*, t. v., p. 120] by Martin-Solon, who made numerous experiments with creasote at his hospital. It is principally from that report that we shall endeavour to indicate the therapeutic properties of creasote, which are really very limited."—P. 180.

The list of diseases and symptoms in which creasote is said by the authors to be more or less useful contains ten items, viz.: 1. Skin diseases and burns; 2. Ulcers; 3. Gangrene of the mouth; 4. Inflammatory affections of mucous membranes; 5. Vomiting; 6. Hæmorrhages; 7. Erectile tumours; 8. Caries of the teeth; and, 9. Parasitical maladies.

The most important remedial action of creasote in medicine is its prevention of nausea and vomiting depending on irritability of the stomach originating in a variety of causes. Yet all that the authors say of its use in vomiting occupies less space than any one of the accounts given of its therapeutic employment in the other eight affections above enumerated; and it is comprised in one sentence to the following effect:—"*Vomiting.*—M. Rayer represents creasote as useful in calming the refractory vomiting frequently met with in Bright's disease."—P. 181. The notice now quoted is most imperfect. Creasote properly administered is second to no article in the materia medica as a remedy for the sickness of renal toxæmia, of pregnancy, of hysteria, and of the latter stages of phthisis. It seldom fails to prevent sea-sickness; and it may be tried with a fair chance of success as a remedy in vomiting from a variety of causes too numerous here to specify. It is sometimes very useful to combine it with drugs which have a tendency to produce nausea; and the alimentation of diphtheritic and other patients with irritable stomach is often much facilitated by administering in a suitable form a suitable dose of creasote at a short interval before giving food. There is nothing new in these and such like statements, as will be evident to any one who will take the trouble to consult Christison or Pereira, printed in 1848 and 1842, or will look at our second volume (p. 943), where there is a paper read by John Rose Cormack before the Medico-Chirurgical Society of Edinburgh on the 6th July 1842, entitled:—"*Contributions towards an Estimate of the Real Value of Creasote as a Therapeutic Agent.*"

The authors of the treatise under review tell us that it was not till 1859 that creasote was introduced into medical practice in France. This statement may be substantially, but is not absolutely correct. For example, so far back as December 1834, a Parisian medical journal—the *Bulletin Général de Thérapeutique*—recorded a case in which Martin-Solon is said to have used creasote with remarkable success in the treatment of a venereal bubo. In

May of the following year, the same journal published a paper by Frémanger on the manner of applying creasote to carious teeth. Creasote was used in France in the treatment of burns, chronic ulcers, and chancres by Berthelot and Goupil in 1833, as we learn from the *Bulletin Général de Thérapeutique* for 1834. In 1835, Breschet announced to the Academy of Medicine of Paris that he had, with great benefit, employed creasote in a case of cancerous ulcerations of the nose in the Hôtel Dieu. We could cite many more examples of creasote having been used as an article of the materia medica in France twenty-five years at least before 1859. It was no doubt very little used in France internally till 1859—a most unfortunate date at which to institute therapeutic researches; for at that time real medicinal creasote could hardly be obtained. At present, so far as we know, it is made by only one manufacturer. Most of the creasote dispensed in the pharmacies of France is the coal-tar creasote, nearly useless, and sometimes worse than useless, as an internal medicine.

From the time that creasote was produced in enormous quantities as an agent for the preservation of timber, and for various other purposes in the arts, it ceased to be prepared from beech-wood. The medicinal creasote was not procurable. For a long time the remedy got a bad name; and we know that even physicians who had long trusted to it in vomiting discontinued to prescribe it from observing that it was no longer reliable. The real state of the case remained long in obscurity. It is now thoroughly cleared up, as is well explained in the following paragraphs which we quote from the introduction to the "Year-Book of Pharmacy for 1872:"—

"Considerable confusion for some time existed between *carbolic acid* and *creasote*, the products respectively of coal-tar and wood-tar. When carbolic acid was first discovered, the two bodies were regarded as identical, and although the constitution of carbolic acid was easily demonstrated, that of creasote remained much longer in obscurity. This confusion led to the substitution of one body for the other; and hitherto much of the creasote of commerce has consisted simply of carbolic acid.

"The researches of Hlasiwetz have shown that true creasote consists chiefly of the ether of creosol, a body having the composition $C_8H_{10}O_2$. Creosol is the homologue of guaiacol, $C_7H_8O_2$, which is formed by the dry distillation of guaiacum resin. Gorup-Besanez states that the several varieties of wood-tar-creasote are mixtures of compound ethers of guaiacol and creosol, the former predominating in Rhenish beech-tar-creasote, the latter in Moravian and English creasote, which are probably prepared from pine-wood tar. Gorup-Besanez also avers that Rhenish creasote contains in addition phenol (carbolic acid). This statement is confirmed by Frisch and also by Marasse. Whether phenol is a normal constituent of Rhenish beech-creasote or not, it is certain that it has been detected in English creasote, which is composed chiefly of a derivative of creosol; and as this body possesses for many purposes antiseptic properties superior to those of carbolic acid, it would appear that the English product should be preferred. Several tests have been suggested from time to time for distinguishing between carbolic acid and creasote. The simplest and best, probably, is that recently published by Mr Morson. He shows that creasote is not miscible with concentrated glycerine, while carbolic acid, as is well known, will mix in all proportions."—*Op. cit.* p. 2.

The creasote prepared by T. Morson and Son of London is obtained from wood-tar only, and differs essentially in its effects from carbolic acid obtained from coal-tar, and now or lately sold by wholesale druggists as "foreign creasote." The creasote which is free from carbolic acid is insoluble in Price's glycerine; and when a specimen does not possess this property it is unfit for internal me icinal employment.

The best forms in which to give creasote are *in silvered pills*, crumb of bread being used as the excipient; in an aqueous solution of gum; and dissolved in water. The pill is generally the best form when there exists great irritability of stomach or a dislike to the odour of the drug. In bronchitis, diarrhœa, and dysentery, particularly when unaccompanied by nausea, the mixture with gum, or the solution in water, generally answer as well or better than the pill. Phthisical patients often acquire a liking for pure or diluted creasote-water as a drink. It moderates the purulent or muco-purulent secretion, and facilitates its expectoration. The creasote-water may sometimes be taken to the extent of a pint, or even more, in the twenty-four hours by chronic phthisical patients, upon whom it often acts as a most useful tonic and calmative. The *mistura creasoti*, originally formulated in the Edinburgh Pharmacopœia,[1] is more agreeable, and sometimes as suitable as the water. In France, creasote is generally given, Trousseau and Pidoux state, in the form of an alcoholic solution; which is an objectionable form, for alcohol increases the pungency of the drug both in odour and in taste. We have heard of physicians who prescribe calcined magnesia as the excipient in the pill, unaware, we presume, that it forms an insoluble compound with creasote.

The *physiological action of creasote* is thus described by the authors:—

"When pure creasote is brought into contact with the skin, it causes violent smarting and slight burning. The mucous membranes are much more affected by it than the skin: they become white, as when touched by nitrate of silver, and the epidermis becomes detached, leaving beneath it the true skin inflamed. Strong creasote-water [*l'eau créasotée, dans une forte proportion*] has evidently an irritant action like the weak acids; but weak creasote-water only determines a constriction similar to that produced by vinegar and other much diluted acids. The astringent effect is succeeded by a real irritative reaction and congestion of a slightly inflammatory character. Creasote, when administered internally, causes an exceedingly disagreeable sensation in the throat, which is neither a sensation of heat nor of smarting, but is a something which recalls the intolerable odour of the substance. When given in too large a dose, it may produce effects similar to those of irritant poisons; and also an obtunding influence on the nervous system."

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[1] The following is the formula above referred to:—"*Mistura Creazoti.*—Take of creasote and acetic acid, of each, 16 minims; compound spirit of juniper (medicated gin) and syrup, of each, 1 fluid ounce; water, 14 fluid ounces. Mix the creasote with the acid; add gradually the water; and add, lastly, the syrup and spirit."

The description now quoted of the physiological action of creasote is too vague and meagre, containing no information which will not be found in the little pamphlet of Miguet, published at Paris in 1834, before the physiological properties of creasote had been fully examined.

To speak of strong and weak creasote water is vague and misleading. Creasote unites with water in two and only two definite proportions. One of the combinations is a solution of one part of creasote in four hundred parts of water, and the other a solution of ten parts of water in one hundred parts of creasote. From the small degree of miscibility of creasote and water the prescriber must take care not to order a mixture in which the drug will float on the top, so subjecting the patient to the risk of taking much larger doses than were intended, causing perhaps headache, nausea, and vomiting, the opposite of the ordinary effects of a therapeutic dose. To prevent such occurrences it is best to use the old *mistura creasoti* of the Edinburgh Pharmacopœia in preference to the *aqua creasoti*, provided there be no objection to it in the particular case under treatment. The acetic acid secures complete solution of the creasote, and its tarry taste is agreeably modified by combination with the compound spirit of juniper. Acetic acid, alcohol, and ether combine with creasote in all proportions.

Properly prescribed, the internal administration of creasote made from beech-wood tar—the only reliable medicinal creasote—is seldom complained of as disagreeable, and never produces heat or smarting. The authors do not refer to the experimental researches of Corneliani at Padua in 1835, and John Rose Cormack at Edinburgh in the same year, into the physiological effects of creasote, which are thus summarized by Christison (*op. cit.*, p. 375):—"In large doses it is a narcotico-acrid poison. Its irritant action when it is taken internally in large doses seems to be veiled or overwhelmed by its narcotic operation, for the leading symptoms of poisoning with it are giddiness, obscurity of vision, depressed action of the heart, convulsions, and coma. Thirty drops will kill a small animal such as a rabbit in a few minutes. When introduced into a vein it occasions instant death by paralyzing the heart. It is equally fatal to vegetable life. It is absorbed in the course of its action, for the urine and breath exhale its peculiar odour for a long time after it is taken. In the human subject its poisonous action has been sometimes manifested by nausea, vomiting, headache, giddiness, heat of the tongue, throat, and gullet, when doses of five, ten, or twenty drops have been inconsiderately given for medicinal purposes."

Adding one short paragraph from page 79 of Cormack's work, we leave this subject:—"From all these experiments, the poisonous effects of creasote appear to bear a very striking resemblance to those of prussic acid. Like the latter, creasote acts toxically either by stopping the heart or by directly affecting the brain alone, just as the dose may be larger or smaller, or according to the manner in which the substances are introduced into the system. Another very striking point of resemblance between prussic acid and creasote is the remarkable and almost specific power which they both possess of arresting vomiting, especially if it be unconnected with organic disease. It is a curious fact that in large doses creasote seems to excite vomiting, and that when given in such quantities, with a view to check it, no good but evil results from giving a large dose."—(*Treatise on the Chemical, Medicinal, and Physiological Properties of Creasote, illustrated by Experiments on the Lower Animals.* Edinburgh, 1836.)

The arrangement of materials in the treatise of Trousseau and Pidoux is convenient; and there is an excellent index of subjects, in which, however, when rigidly tested, omissions are detected. For example, under the word VOMITING, *Vomissements*, a long list of remedies is given, but *cerium* is not there mentioned, although inserted in the alphabetical index, which refers the reader to p. 518 of the second volume, where we read:—"Professor Simpson of Edinburgh administered the nitrate and the oxalate of the oxide of cerium as sedative tonics. He employed them in the dyspepsia which is accompanied by gastrodynia and pyrosis, in chronic vomiting, and particularly in the vomiting caused by pregnancy. It is administered in water in doses of five centigrammes (rather less than a grain) three times a day." We may remark that considerably larger doses are generally required, and that it may be otherwise given than in water. Twenty centigrammes every two, three, or four hours are often administered with success when doses of five centigrammes are useless. The oxalate of cerium may be given in pill, in mixture with tragacanth powder, or in water; but when retching is continuous, the best plan is to place three grains dry on the tongue, together with a bit of ice.

The first volume of the treatise contains a hundred pages of admirable introductory matter, written by Pidoux, and six chapters. The second volume contains eight chapters and a supplement on Posology. The subjects of the fourteen chapters are respectively as follow:—1. Reconstituants; 2. Astringents; 3. Alteratives; 4. Irritants; 5. Antiphlogistics; 6. Evacuants; 7. Excitants of the

Muscular System; 8. Narcotics; 9. Anæsthetics; 10. Antispasmodics; 11. Neurosthenic Tonics; 12. Excitants; 13. Sedatives and Contra-Stimulants; 14. Anthelmintics. The twelfth chapter includes general and special "excitants;" the latter class embracing, in separate subdivisions, sudorifics, diuretics, emmenagogues, and balsams. Each chapter contains not only a description of the properties of each of the articles of the materia medica which belong to it, but likewise an interesting essay on the history and particular characteristics of the medication to which they are applied.

Prefixed to the hundred pages of introductory matter we find the following notice by Pidoux, its author, dated February 1875:—

"The first volume of our last (8th) edition was scarcely completed, when Trousseau died at the age of 65. Having observed that for some time therapeutics had been invaded and dominated by principles derived from other sources than medical art and clinical observation, he was desirous that I should embody in a general introduction to our work the considerations herewith presented. I prepared in haste the introduction, and submitted it to him: it met with his entire approval. These were our latest scientific relations. On the 30th May 1867, I left for the Eaux-Bonnes: our parting embrace was in silence, both knowing well that we should never meet again. He died on the 23d of June. Under these circumstances, I have thought it right to reproduce the introduction at the commencement of the ninth edition. Moreover, I look upon it as being even more opportune now than when first published seven years ago."

In conclusion, we have to congratulate Drs Pidoux and Constantin Paul upon the able manner in which they have accomplished their arduous task by bringing out a ninth and improved edition of their great work. Its faults and imperfections—our criticisms notwithstanding—are comparatively few; its excellences are many and conspicuous. It is a well-arranged encyclopædia of therapeutics and the materia medica, the result of industrious research and discriminating compilation, strengthened and moulded into shape by clinical experience. English physicians who wish to maintain an accurate acquaintance with the views current in France on therapeutics, the materia medica, and pharmacology, ought to add it to their libraries. French physicians, however, must not accept the work as an accurate or complete guide to British therapeutics.

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*Annuaire de Thérapeutique de Matière Médicale de Pharmacie et d'Hygiène pour* 1877. Par A. BOUCHARDAT et J. BOUCHARDAT: $37^{me}$ année: 16mo, pp. 270: Germer Baillière et fils: Paris, 1877.

THIS, the 37th volume of Bouchardat's annual retrospect of Therapeutics, Materia Medica, Pharmacy, and Hygienics, fully maintains the high character of its predecessors, containing, like them, a great deal of valuable and well-arranged matter. It gives a critical abstract of works, great and small, on therapeutics and hygienics published during 1866, and furnishes formulæ for new medicines. The materials are derived almost entirely from French sources, so that the titlepage suggests a more comprehensive book than the reality justifies. There is added to the general matter an interesting essay, entitled, "General Considerations on Etiological Therapeutics." The price of this excellent little volume is one franc and a half.

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*Traité du Microscope et des Injections et leur Emploi; de leurs Applications à l'Anatomie Humaine et Comparée, à la Pathologie Médico-Chirurgicale, à l'Histoire Naturelle Animale et Végétale, et à l'Economie Agricole.* Par CH. ROBIN, Membre de l'Institut et de l'Académie de Médecine; Professeur d'Histologie à la Faculté de Médecine de Paris. *Deuxième édition revue et augmentée: avec* 336 *figures intercalées dans le texte et* 3 *planches gravées.* Pp. 1101. J. B. Baillière et Fils: Paris, 1877.

THIS second edition of Professor Robin's classical work will be received with much satisfaction by histologists, and by all who, for any scientific purpose, use the microscope. It gives much valuable information not contained in the first edition, published in 1871; and by an improved arrangement of the matter, it will now be much more easily consulted than formerly. No other work gives so complete an account of microscopes and the many ways of working for them and with them.

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*Eighteenth Annual Report of the General Board of Commissioners in Lunacy for Scotland.* 1876.

THE most apparent improvement in this number of the Report is a diminution in its size. From the nature of things, a blue book of this kind can never be a very readable production, even supposing the Commissioners were to adopt the advice which Mark Twain puts in the mouth of one of his humorous characters, that such public documents should be interspersed with rhymes and conundrums as an artistic relief to the general dreariness of the contents.

It appears that of insane persons in Scotland 1476 are maintained from private sources, 6700 from parochial rates, and 49 at the expense of the State. Besides this, there are all the unreported insane maintained by their friends in private dwellings, whose number can only be guessed at. In 1858, it was estimated at nearly 2000, but it is likely that out of this class a large number of pauper lunatics have been drawn.

From Tables given in the Report, it appears that the proportion of ordinary paupers to the population has decreased from 2555 per 100,000 in 1861, to 1958 in 1875, while pauper lunatics have increased during the same time from 171 to 191 per 100,000. The increase in the admission of private patients is very little higher than the increase in the population since 1858.

Most people will be disposed to accept the explanation of this increase of pauper lunatics thus cautiously put forward in the Report.

The number of pauper lunatics admitted into asylums and other establishments for the insane since 1858, has increased at the rate of 63 per cent., while the general population has only increased at the rate of 15 per cent. The increase in 1875 was 148.

"It does not necessarily follow," adds the Report, "from these facts that there is any greater production of insanity in the country. It is possible that they only express what might be looked for as the result (1.) of the increased facilities of obtaining gratuitous treatment and maintenance in asylums; (2.) of the diminished dislike to resort to the treatment of insane persons in asylums; (3.) of the increased readiness to adopt the more costly forms of treatment, in consequence (*a.*) of the greater wealth of the country, which affects the admissions both of private and pauper patients, and (*b.*) of the relief to local taxation from imperial sources, which affects only the admissions of paupers; (4.) of the altered medical and public opinion as to what constitutes lunacy, especially as to what constitutes that degree or kind of lunacy which can be certified as rendering the subject of it a fit and proper person to be placed under care and treatment in an asylum.

Perhaps the large increase in the admissions of pauper patients during 1875 is to some extent the outcome of the grant from Government towards the maintenance of pauper lunatics. We cannot yet speak with any certainty of the practical working of the grant; but it scarcely admits of doubt that the relieving of local taxation, in such a matter as lunacy, must have a tendency to lead parochial authorities to include among pauper lunatics persons who would not be reckoned as lunatics if the whole burden of their maintenance fell directly and exclusively on the rate-payers of the parish. It is too apt to be forgotten that the statutory lunacy of a country is far from being a thing which "human power cannot multiply and modify." In our Fifteenth Report (p. 285), we pointed out that "the existence of lunacy, in so far as it is officially recognised or required to be dealt with by the State, is at present decided by the certificate of two medical men; and indeed must always be determined in that or some similar manner. If there be persons who imagine that a uniform standard of mental soundness is accepted by all medical men, or by any one medical man, in all circumstances, they must have little experience to guide them. Such certificates are always signed after a consideration of the social as well as the medical circumstances of each case. And it is scarcely open to doubt that in actual practice the source from which the required expenditure is to be obtained, is, rightly or wrongly, a common element in this consideration."

We fancy that most people who attend to the subject are coming to the conclusion that this increase of insanity is mainly owing to unskilful legislation for the insane. The state of affairs, as may be gathered from the Report of the Scottish Commissioners, seems to be this:—The number of pauper lunatics is steadily increasing, while the number of sane paupers is diminishing. The new asylums are getting overcrowded and need extension; the old asylums are not up to modern wants, and new ones must be built; better furniture and more decorations are still needed; the wages of attendants require farther increase; the Commissioners are getting the locks of the asylums remodelled, and recommend what is called the open-door system, with the view of adding to the delusions of the insane the pleasing and perhaps salutary delusion that they are at liberty. The sane have sometimes their delusions too, and perhaps one of them may be that all this is like filling at the small end of the horn to let it run out at the large one. The number of certified lunatics is getting larger and larger every year, only their bonds are made lighter. There are fewer in the airing courts and fewer under seclusion than formerly, but there is a large number more of those unpaid *attachés* who cultivate the farms round about the asylums without running away, or at any rate without running away so fast that they cannot be caught by the keepers who watch them.

It is well known to every medical superintendent how the governors of poorhouses can handle the Lunacy Acts to get rid of the troublesome, crazy, or irritable people whose conduct furnishes some "fact indicating insanity," or of those whose bodily decline is accompanied with symptoms of mental weakness, or of helpless paralytics in whom some shades of mental infirmity can be found, and who are sent into asylums under medical certificate, more because they are troublesome to their relations and guardians than because an asylum is a fit place for them. We do not say that the Board of Lunacy is responsible for these evils, which indeed they have again and again pointed out. We cordially agree with them that the Government grant to pauper lunatics should be abolished. So far from paying one-third of the board of pauper lunatics out of imperial rates being any saving to the nation, or any relief to those who deserve relief, it rather helps, as the Report shows, to make the burden heavier, by holding out a temptation to thoughtless expenditure. Besides, a practice has grown up within a few years of inspectors of poor arranging with people who are able to pay the whole or part of the board of their insane relations in order to get them into the public asylums. These are, of course, only nominal paupers, and it is difficult to see how it can be arranged that one-third of their board should not be paid out of the State budget. This perhaps was more than the Chancellor of the Exchequer ever had in view.

It seems very desirable that there should be less vague definitions of what insanity is, and what forms of insanity and mental weakness should go into asylums; and it does not seem likely that without remodelling of the Lunacy Acts some evils in their working can be successfully grappled with. We should be disposed to add to the powers of medical superintendents and physicians in charge of lunatic wards as to the reception and disposal of unfit patients. These men have much too weak a position and too insecure a tenure of office for their great responsibility. They are swayed to and fro between the District Boards and Committees and the Central Lunacy Board. To deprive them of the power of acting in difficult circumstances is like tying the hands of the helmsman, in the hope that a vessel will be guided by favourable winds and tides into the port. In making these remarks, we trust that we are not forgetful that the Lunacy Acts ought to have for their main object the welfare of the insane and the interests of the general community, and not the dignity or profit of medical men. On the whole, it may be said that the Commissioners in Lunacy seem to have a praiseworthy sympathy for the class of which the law has made them the protectors.

The Board has made a good use of the statistics in its possession to prosecute an inquiry into the geographical distribution of general paralysis and delirium tremens. We give the results in the words of the Report:—" It appears that the higher mortality characteristic of town populations manifests itself in a special manner when we look at the statistics of all nervous diseases, but still more remarkably in regard to delirium tremens and general paralysis. This comes out still more distinctly if we consider the number of deaths in the towns from these several causes, proportionate to 100 deaths from each cause in the insular and mainland-rural districts. Calculated in this way, we find that for every 100 deaths from all causes in the rest of the country, there are in proportion to population 159 deaths in the towns. For every such 100 deaths from nervous diseases there are 170 in the towns. For every 100 deaths from delirium tremens, there are 217 in the towns. And the corresponding proportion for general paralysis is 237. We have here a remarkable indication of the special prevalence in towns of diseases of the nervous system. This no doubt results partly from the greater strain which town life makes upon the nervous and mental energy, but probably in a still greater degree from the injurious influences of imperfect sanitary arrangements and hurtful social practices; and it is interesting to find the opinion that delirium tremens and general paralysis are special products of the dissipation and feverish activity of town life so strikingly corroborated by the figures. We have thought it desirable to exhibit the relative proportions of delirium tremens and general paralysis, because general paralysis is believed by some authorities to be due to excessive indulgence in alcoholic and cognate stimuli, and the parallelism in their geographical distribution affords some countenance to this view."

*Otological Memoranda: being Clinical Observations illustrative of the Diseases and Injuries of the Ear.* By JAMES PATTERSON CASSELLS, M.D., M.R.C.S. Lond.; Fellow of the Faculty of Physicians and Surgeons; Surgeon to, and Lecturer on Aural Surgery at, the Glasgow Dispensary for Diseases of the Ear. Glasgow: Dunn and Wright. Reprinted from the *Glasgow Medical Journal*, April 1876.

WITH this paper of eighteen or twenty pages Dr Cassels commences the first of a series intended to appear from time to time in the pages of the *Glasgow Medical Journal*, and, we hope, to be re-issued like to the present in form. Dr Cassells commences "On malformations of the auricle and external auditory meatus, congenital and acquired; Injuries of the auricle and meatus; Value of early incision in the treatment of the inflammatory affections of the meatus; Injury of the mastoid and rupture of lateral sinus." With regard to the first three heads, we should advise those who have to treat such cases as Dr Cassells relates to consult "Diseases of the Ear," Toynbee, 1868, chapter ii., especially for cases of malformation of the auricle and external auditory meatus. The fourth head—Injury of the mastoid and rupture of the lateral sinus—is illustrated by Case 9; a case recording a remarkable injury and cure, to a healthy girl aged seven years, who was knocked down and trodden upon over the mastoid process by the hoof of a horse recently shod. Dr Cassells fortunately happened to be passing, and seeing that a stream of blood of about the circumference of the child's forefinger was freely flowing from a wound in the mastoid process, successfully arrested the hæmorrhage by having tampioned the wound as well as the external auditory meatus; and after restoring the child in a neighbouring house had her conveyed home, where she lay in a highly critical condition for eight days. At the end of that time the first dressing was removed; no bleeding took place, and there was found a circular wound half an inch in diameter in the mastoid region communicating with the external auditory meatus. So far as could be ascertained no injury had happened to the tympanum and membrana tympani. Pieces of bone were discharged by the external auditory meatus, but the patient made a good recovery. "The point alone unexplained was—What was the source of the dark stream of blood?" The case deserves to be kept on record on account of the prompt means used by an energetic and kind-hearted doctor.

We welcome the series of papers promised; such records and remarks will hasten the establishment of Otology as a legitimate specialty.

### *Journal of the Scottish Meteorological Society, with Tables for the Half-Year ending 31st December* 1875. William Blackwood & Sons: Edinburgh and London.

We have much pleasure in highly recommending this Journal to the notice of our readers. It contains the record of what is being done in this department of science by the Scottish Meteorological Society. This Society is doing much for the advancement of this department of science, due, in a great measure, to the energy and ability of Mr Buchan, the eminent Secretary of the Society, and who has done more than any man in this country to place meteorology on a true scientific basis.

The present numbers contain several very interesting and important papers. We can only notice a few of these.

The "Report of the Ozone Committee" will be read with interest by all members of the medical profession. "The Flowering of Spring Plants," by Mr Buchan, is a most instructive and scientific paper, from which we extract the following:—"It will be observed that the temperature falls to the lowest point on the 11th January, which may be regarded as the turning-point in the winter temperature, since, previous to this date, the temperature is on the whole steadily falling; and after this date, it continues steadily to rise. Further, previous to the 11th January the weather is characterized, to a large extent, by a prevalence of cloud, under which, though the sunshine is less, yet a higher temperature is maintained. But after the 11th January the rainfall becomes less, clear weather is more frequent, and the increase in the temperature which sets in is mainly due to an increase of sunshine and the increasing power of the sun's rays in their influence on vegetation. The 11th January may thus be regarded as a starting-point for vegetation."

Mr Buchan has given us some interesting information with reference to the relation which the colours of flowers have to the date of flowering. He shows that "the *blues* are, on the average, considerably the earliest in flower; then follow in order the *whites* and the *purples*, and, lastly, the *yellows* and *reds*. It follows that the plants included in the British Flora clearly tend to arrange themselves, as regards the dates of flowering, in the colours of the spectrum, the average earliest being those which are nearest the part of the spectrum where the actinic rays are at the maximum."

The Journal also contains many other very interesting papers, besides most important meteorological tables. We commend it very highly to all our readers.

*How to Use a Galvanic Battery.* By HERBERT TIBBITS, M.D., etc. J. and A. Churchill: 1877.

WHOSE starch do you use? Lawrence Hall's to be sure; we use the best of everything. Dr Tibbits' discourse is framed rather too much on the model of this railway-station advertisement. The Tibbits' voltaic battery, the Tibbits' induction battery, the Tibbits' hospital combined battery, the Tibbits' portable combined battery, the Tibbits' hospital galvano-caustic battery—are the awful names under which he lauds his inventions. Nevertheless, Dr Tibbits makes out a very good case for his productions. There is no doubt that the Leclanché cell which he adopts is the most convenient yet produced, and the arrangements of the batteries are probably at least as good as any which have been introduced to the profession.

The rest of the lecture, if it contain nothing very novel, is a fair, temperate, and clear exposition of the departments of electro-therapeutics which he considers.

From want of space we have unavoidably to postpone some excellent reviews now in type, including an elaborate notice of Dr Magnan's "Recherches sur les Centres Nerveux."

# Part Third.

## MEETINGS OF SOCIETIES.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION LVI.—MEETING VI.

*Wednesday, 7th March* 1877.—Dr GILLESPIE, *President, in the Chair.*

### EXHIBITION OF PATHOLOGICAL SPECIMENS, PATIENTS, ETC.

I. *Mr Annandale* showed—(1.) a GIRL whose knee-joint he had excised five years ago. As is well known, surgeons, in this operation, aim at anchylosis of the cut bony surfaces. This case, however, and one shown at the Clinical Society of London by Mr Barwell, were the only two recorded—as far as he was aware—where a movable joint was also a useful one. As they could see, the patient had motion at the site of excision and could walk well. The affected limb was an inch and a half shorter than the other.

The *President* asked if the mobility was intended or accidental? In either way the result was magnificent.

*Mr Annandale* explained that it was not an intentional result.

(2.) A SPECIMEN OF DISLOCATION OF THE HEAD OF THE FEMUR

FROM DISEASE. The interesting point was that the amount of disease was not great, and was due to a secondary suppuration of the joint following on a psoas abscess. The capsular ligament was in part destroyed, but the Y-shaped ligament of Bigelow was left. The displacement was, as they saw, an unusual one, and was caused by the patient lying, before death, with his thigh flexed.

(3.) An interesting specimen in connexion with HIP-DISEASE. There was evidence of active inflammation in the joint, but none of external suppuration. He incised antiseptically, and, finding a carious cavity in the neck of the femur, he removed the head and neck of the bone. The point he wished to direct attention to was that the disease had commenced in the epiphysis and had resulted in necrosis. It was quite evident that unless the necrosed bone now shown, which was lying loose in the cavity, had been removed there would have been profuse and continued suppuration.

(4.) SPECIMEN AND PHOTOGRAPH FROM A CASE OF DISLOCATION AT KNEE-JOINT FROM DISEASE.. The tibia was displaced back and the limb useless. He first divided the hamstrings, but without effect. He then made a free lateral incision, and sawed off the condyles of the femur with the anchylosed patella. He was, however, still unable to get the limb straight until he cut off another slice. He did not interfere with the tibia. This was now the second time he had operated in this manner and with a good result on both occasions.

(5.) A GAG for keeping the mouth thoroughly open in operations upon the tongue or other parts of this cavity. The best for this was the late Sir William Fergusson's. In using it he had, however, found two disadvantages, viz., the handles were in the way when the patient was lying, and the instrument was apt to slip. He had therefore had made and used this instrument, which was a combination of Sir William Fergusson's and Mr Hutchinson's. It had handles which could be removed, and also a mould for the teeth.

II. *Dr Argyll Robertson* showed—(1.) an EYEBALL removed from a woman three weeks ago on account of a tumour in its interior. The growth had penetrated the sclerotic, close to the inferior margin of the cornea. Three years ago her eyesight had failed; and a year afterwards she noticed a small, dark-coloured speck, where subsequently the tumour protruded. One-third of the vitreous chamber was occupied by a growth, which, when examined by Dr Wyllie, was found to be melanotic cancer, there being large pigmented cells undergoing endogenous development. It was interesting that although present for at least two years, the tumour only filled one-third of the vitreous chamber. From its being thus limited, and as the optic nerve on section was healthy, he hoped the disease had been eradicated.

(2.) A TUMOUR OF THE ORBIT removed from a boy ten years of age. It caused extensive protrusion of the eyeball, and had been growing for three years. It extended back to apex of orbital cavity, and was intimately associated with the optic nerve, which was lost in its substance. Yet the boy could count fingers held before the eye, although the whole nerve was so implicated. From this and the fact that the movements of the eye were unimpaired and the tumour not connected with the periosteum, he took a favourable view of its nature. This was confirmed on microscopical examination, as it was found to be a simple fibrous growth.

III. *Mr Bell* related the following CASE:—On Saturday night a young gentleman at the Gymnasium had gone outside the railing of one of the swings. When there he got confused, lost his head, and was thrown out, alighting on a paling. As he was insensible, he was brought into one of his wards, when the following was found to be the nature of the injury: A large piece of paling, pointed at one end, 9 inches long and 3 inches broad, and ½ an inch thick, had penetrated about 3 inches below the axilla and passed up between the scapula and chest wall, appearing at the acromion process. It fortunately did not pierce the skin there; and the subclavian vessels were uninjured. He dilated the wound; washed it out with carbolic lotion one to twenty; and dressed it with antiseptic precautions, using free drainage. The patient was now doing well, and the wound was quite sweet. The case was interesting from a medico-legal as well as from a surgical aspect, inasmuch as small pieces of all the layers of clothes he had on at the point of transfixion were found on the top of the paling. When the patient was picked up at the Gymnasium the proprietor did not know that there was such a large piece of his paling in him, because only a small part projected at the wound.

IV. *Dr G. W. Balfour* showed a HEART. Four months ago the patient had been admitted into his ward with a distinct diastolic thrill in the aortic area, the subclavian, carotid, and brachial arteries, and even low down in the abdominal aorta. There was a loud, musical, diastolic, aortic murmur. Cases of diastolic fremitus were not common. Walshe had recorded only one, while Hayden says he had never met with one, but gives an account of a case.[1] He himself did not believe it was so rare, as he had seen two in Professor Maclagan's wards, and at present had four under his care with diastolic thrill and loud diastolic murmur. In one the murmur was so loud that the patient's wife could hear it across the dining-table; indeed, he felt ill unless it was so heard. It was evident that in such cases there was something projecting across the current of blood. Sir Thomas Watson relates one such case,

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[1] In his recent work on Diseases of the Heart, Dr Hayden says, at p. 840: "I have not met with a single instance of it (fremitus) in connexion with the murmur of aortic reflux, and I incline to believe that it is never so associated." At p. 843, however, he records a case with a soft, blowing, systolic murmur and a jarring, faintly-musical, diastolic murmur, the basic fremitus of which must have been diastolic.

in which the inner half of all the segments hung loose, producing the vibrations. In the case now shown one segment of the aortic valve was so depressed. There was no systolic murmur over the cardiac base, but only in the arteries. The diastolic murmur was loud and musical. The aorta was found to be dilated, all the segments of its valve sufficiently healthy to present no obstacle to the egress of the blood, but one had been so torn from its attachments as to be readily depressed by the descending current across which it projects, and by which it was thrown into the vibrations which produced both the fremitus and the musical murmur. The case was interesting, too, because digitalis had been given freely and for a long time. Two of Nativelle's digitaline granules (one granule = 10 mm. of tincture) were given every four hours. Every now and then the urine fell, and the question arose, Whether this was due to too much or too little digitalis? For reasons he would not give now, the dose had been increased on every such occasion to three granules every four hours. The day before the patient died he left orders to increase the dose; but as the patient was sinking rapidly this had not been done. On post-mortem the heart was found in diastole, so that it was clear that the patient died from want of digitalis, not from too much. It was interesting that such large doses were borne not only without injury, but with benefit. This marked a great change in their ideas, as he recently met a medical man who remembered a case which occurred in the Edinburgh Infirmary about forty years ago, where digitalis was being given in four-minim doses of the tincture. Death occurred suddenly, and was attributed to the accumulative action of the drug.

V. *Dr Byrom Bramwell* showed specimens of ANEURISMS ON TERMINAL BRANCHES OF THE PULMONARY ARTERY IN THE WALLS OF CAVITIES, and said—Since the publication of Rasmussen's paper,[1] the attention of physicians and pathologists has been directed more closely than before to the source of the hæmorrhage in cases of fatal hæmoptysis, with the result that Drs Douglas Powell, and Green of London, Coats of Glasgow, Franks of Dublin, and other observers, have confirmed the author's statement that in many instances the source of the hæmorrhage is a ruptured aneurism of a terminal branch of the pulmonary artery. The four specimens which I have the honour to show you this evening are examples of this somewhat rare condition. I lately met with them in my capacity as Pathologist to the Newcastle-on-Tyne Infirmary.

(1.) The first patient, a coloured man, æt. 21, was admitted under the care of my colleague Dr Gibson (by whose kind permission I am enabled to bring the case before you), suffering from extensive pulmonary disease, more marked on the right than on the left side. While in hospital he had three terrible attacks of hæmorrhage, expectorating on each occasion between thirty and forty ounces.

[1] Edin. Med. Journ., Nov. 1868, p. 385.

And it is a clinical fact, which, perhaps, is worthy of mention, that before each hæmoptysis there was a marked fall of temperature; indeed, so striking was it that our junior house-surgeon, Mr Dixon, was enabled to predict the occurrence of the last attack. The man died some thirty hours after the last bleeding. At the autopsy I found extensive tubercular disease of both lungs. The right apex was hollowed out into a cavity the size of an orange. At the inferior part of this cavity, the walls of which were dense and cicatricial, there was a small aneurism (see Fig. 1) the size of a pea. It was unruptured, and was situated on a minute branch of the pulmonary artery. A second smaller cavity in the middle of the lung was filled with coagulated blood, partly recent, partly old and decolorised. A second aneurism (Fig. 2) the size of a cherry was found in the cavity. The aneurism had ruptured by a slit-like opening about three lines in length. You will see the point of rupture; it is now considerably larger than it was at first. This aneurism was evidently the source of the hæmorrhage. Firmly adherent to the outer surface of the sac, and partly blocking the ruptured orifice, was a portion of the decolorised clot. You may perhaps notice that at one part the outer wall of the aneurism looks rough. At that point a portion of the clot is still adherent; but owing to the action of the spirit in which it has been preserved, it is now quite blanched and unrecognisable. Still lower down there was a third aneurism (Fig. 3), larger than the others and more irregular in shape. This aneurism almost completely filled the cavity in which it was situated. The wall was at one point very thin, but it was unruptured. It was situated on a fair-sized branch of the pulmonary artery. The cavity of the aneurism was divided by a well-marked septum into two parts. From the lower end of the sac two efferent vessels spring. I have made a section of the aneurism, and passed a piece of straw through the branch of the pulmonary artery from which it arises, through the cavity of the aneurism, and into one of the efferent vessels.

(2.) The second patient, a man æt. 28, was admitted under the care of my colleague Dr Philipson (who has very kindly allowed me to use his notes), suffering from tubercular phthisis. On 15th December, while Dr Philipson was in the ward, he was attacked with violent hæmoptysis, the blood spurting from the mouth and nose. He was dead in a few minutes. At the post-mortem we found both lungs infiltrated with tubercle. In the left apex there was a cavity the size of a small apple, and in this cavity was situated a round aneurism (Fig. 4) the size of a cherry, very like the second specimen I have shown to you. The aneurism had ruptured by an opening sufficiently large to admit of the passage of a good-sized probe. The cavity in which it was contained, the bronchial tubes of both lungs, and the trachea, were filled with blood, partly fluid, partly coagulated.

Fig I.

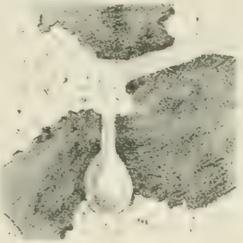


Fig II.

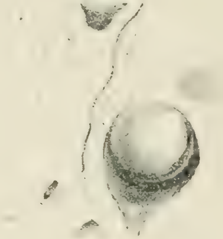


Fig III

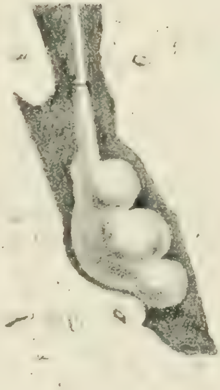
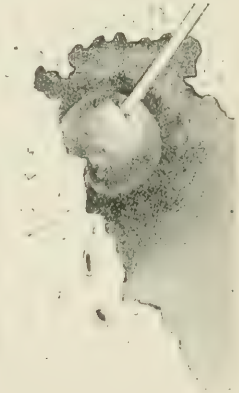


Fig IV



M B Del. [illegible] lith.

All four aneurisms contained a small quantity of fluid blood. In none was a clot found.

One of these patients had suffered from syphilis, a fact which is perhaps of some importance, for the close connexion between syphilis and aneurisms of the arterial system generally is now acknowledged by most authors. I should like to ask the members of this Society, whose experience is greater than my own, whether there are any facts in support of such a connexion in the particular form of aneurism I have been describing? I may mention, that in both patients the general arterial system was healthy. In one of the cases the previous history with regard to syphilis was not ascertained.

The *President* remarked that while no discussion was allowed on pathological specimens, the Society was much obliged to Dr Bramwell for his kindness in coming such a distance to show his beautiful specimens.

### DESCRIPTION OF PLATE.

FIG. 1. Small aneurism unruptured (Case No. 1).

FIG. 2. Aneurism which had ruptured and was the source of the hæmorrhage in Case No. 1.

FIG. 3. Irregular sacculated aneurism from Case No. 1. A probe has been passed into the branch of the pulmonary artery from which the aneurism springs. Aneurism is situated in the cavity of a dilated bronchus.

FIG. 4. Aneurism from case No. 2. A probe has been passed into the ruptured orifice.

*Note.*—The Aneurisms are drawn of natural size. Portions only of the cavities are seen.

VI. *Mr Chiene* showed a SPRAY-PRODUCER made by Gardner, instrument-maker, under his superintendence. Its advantages are—*Lamp.* (1.) Was simpler in mechanism than any he had seen. (2.) Not liable to go out of order. (3.) Giving a strong flame, with small consumption of spirit; quickly and easily altered in volume. (4.) Does not heat the spirit in the body of the lamp. After three hours, the time the spray lasts, the temperature of the spirit is 85° to 90° F., attained principally by porcelain supports. *Boiler.* Simplified by making the opening for the safety-valve and the opening for pouring.

VII. *Dr Turnbull*, Coldstream, read a paper on a CASE OF COLICA PICTONUM DUE TO SWALLOWING A LEAD BULLET, which appeared at p. 897.

The *President* thought that the Society was much indebted to Dr Turnbull for his valuable paper. He regretted that there was no *post-mortem*, as it would have given much more satisfaction. At the same time it was a valuable case, because many surgeons had seen instances where halfpence and pence were swallowed without harm. He himself had seen two such; and there was no reason why copper should not prove fatal as well as lead. The new way to get lead-poisoning was by drinking soda-water. He had lately read a case in the *Medical Gazette*, where a tailor got it by putting his measure, which was covered with enamel and white lead, into his mouth. Probably the action of no poison had been more investigated than that of lead, and the paper to-night was a valuable addition to its literature.

*Mr Annandale*, along with others, felt the Society was much indebted to Dr Turnbull for his paper. After listening to the graphic account of the case, while he believed that the bullet was the cause of death, he could not yet coincide with the explanation given. The bullet probably lodged in the intestine, caused a twist because of its weight, and thus produced obstruction. Against the theory of lead-poisoning it could be urged that it was never so rapid, and therefore he believed death was probably due to obstruction.

*Professor Sanders* remarked on the great interest of the paper, as the accident was one likely to happen to children, and the symptoms were of a most alarming nature, and with a rapid and fatal issue. He had never read such a case. As to its nature it was one of difficulty. The symptoms were highly inflammatory, as they could see from the temperature of 102° and the high pulse. Such was not the case with lead-poisoning, which was an anti-pyretic disease. The symptoms of violent colic left it an open question whether there was obstruction or lead-poisoning. He was not prepared to say which it really was. As to Mr Annandale's suggestion, if he followed the case aright, the bowels were cleared by purgatives, and there was no stercoraceous vomiting. It appeared most probable that the bullet lodging caused inflammatory action, and therefore ulceration, as was shown by the vomiting of blood. The case was one where a *post-mortem* would have been of the greatest value. Nevertheless, even without that, it was extremely instructive. He did not think that simple lead-poisoning would cause so rapid a death; and he therefore believed that death resulted from inflammation caused by the bullet. The case was one well worthy of their consideration.

*Dr Wyllie* inquired as to the seat of the pain. From observations he had made in the *post-mortem* room as to the position of the stomach, he believed it probable that in Dr Turnbull's case the bullet remained in the stomach and set up an inflammation there, which led to ulceration and hæmorrhage.

*Mr Bell* thought it extraordinary that the bullet did not pass the ordinary way. He agreed in the main with Professor Sanders and Dr Wyllie, but thought the bullet had probably lodged in the vermiform appendix. Although no chemist, he believed that the presence of the products of inflammation would cause a more active absorption of the lead into the system. Dr Turnbull had stated that he was averse to the publication of isolated cases; but he hoped that he would again favour them with some of his great experience.

*Dr Carmichael* thought that most of the points had been touched on. It struck him, however, that the suddenness of the change pointed to perforation and collapse following on ulceration.

*Dr A. G. Miller* had had his curiosity excited as to the treatment.

Mr Bell's remarks suggested to him that the case would have had a better termination if the boy, instead of being kept quiet, had been made to rush about. In such cases it would probably be advisable to do this, so as to encourage the passage of the bullet downwards.

*Dr Keiller* thought the boy should have been turned upside down.

*Dr Turnbull* had felt the case to be a difficult one. Although he had applied to many of his friends he had been unable to get any record of one like it. He would have been glad to have got an explanation of the strangury from Professor Sanders. Such strangury and emaciation he had never witnessed. Another point on which he would have liked information was the advisability of giving an emetic. In one case castor-oil was given with a good result. One thing he was perfectly confident of was, that the boy did not die of obstruction. The frequency of cattle being poisoned with the lead-spray from targets, as pointed out by Mr Robertson of Kelso, was an interesting fact and had a bearing on his case. In conclusion, he would only say, that to attend such a case, twelve miles from home, with his other practice in addition, was no easy matter.

In reply to *Dr Keiller*, *Dr Turnbull* stated that the parents were averse to any *post-mortem* examination, and the procurator-fiscal did not authorize it. Under such circumstances he did not feel it his duty to press for it.

VIII. *Dr Paton* then read a paper on EXPERIMENTAL RESEARCHES ON THE ACTION AND SOUNDS OF THE HEART, which appeared at p. 902 of the April number of this Journal.

*The President* remarked that the Society was indebted to Dr Paton for his well-argued paper, in which he tried to upset the views presently held as to the causes of the heart-sounds.

*Professor Sanders* said that every now and then discussions as to the causes of the heart-sounds were cropping up. The theory brought forward by Dr Paton was not a new, but an old one, as it had been advanced by Laennec. There was one observation, however, that he would like to make, which was that, if the second sound depended on the contraction of the auricles, then, in constricted disease of the mitral valve, it should be altered. But, instead of this, they had a pre-systolic murmur, and then the first sound, followed by the second, stronger than usual. He could not see how a lesion which, in Dr Paton's theory, would really abolish the second sound, should only have the effect of strengthening it. No fact was better established than that the second sound depended on the closure of the sigmoid valves. The paper was ingenious, and they should keep in mind that a truth was not a truth unless it could stand attacks. From all these facts he thought he could give Dr Paton no better advice than that of the old saying, *stare super vias antiquas*.

*Dr G. W. Balfour* thought Professor Sanders was not quite happy in his quotation, as it was really an old view Dr Paton had brought forward. Laennec held that the contraction of the auricles caused the second sound. Dr Paton had been long known to himself as an enthusiastic experimentalist in search of truth, and, twenty years ago, he had read a very remarkable experimental paper by him on the perceptive power of the spinal cord. Both it and the communication to-night showed a desire in Dr Paton to obtain truth. He agreed that Dr Paton was trying to upset all their present ideas as to the sounds of the heart; and he also concurred with Professor Sanders in holding that the best proof of Dr Paton's error was to be got from a consideration of the phenomena accompanying mitral stenosis. In many cases of this lesion, where the auricle is dilated and hypertrophied, they could feel and see its pulsation, and note that the auricular systole preceded the ventricular one, and that the second sound invariably followed the ventricular systole, and coincided with the ventricular diastole. He could not therefore acknowledge that there was any truth in Dr Paton's theory that the second sound was caused by the auricular contraction. Instead of quoting to him the phrase *stare super vias antiquas*, he would alter it to *stare super cor morbidum*. It was easy to see how Dr Paton's zeal for observation had led him astray. When the heart was once denuded, all the valves were so close that the stethoscope covered parts of all, and therefore it was very difficult to determine which was the first and which the second sound, even in the human heart, when we had only one set of semilunar valves, and in the turtle, on which Dr Paton had experimented, and in which there were two sets of semilunar valves, he could imagine that the difficulty was very much greater. In the human heart this question could always be settled by reference to the silences. There was the short silence between the first and second sounds, and the long silence between the second and first. The other grave error was in concluding that the aorta required to contract to complete the pulse-wave. The pulse-wave really passed as a tidal-bore-wave due to ventricular contraction alone, the pulse in every part of the body preceding the reaction of the aorta, by which the aortic valves were closed, and the second sound produced, and the cardiac arteries flushed with nutrient blood during the diastole of the cardiac muscle.

*Dr Smart*, in a long speech, combated Dr Paton's views, and pointed out, that when the sigmoid valves were hooked back the second sound ceased. The first sound was produced, not by the contraction of the ventricle, but by the closure of the valves and the vibration of the chordæ tendineæ.

*Dr Bramwell* pointed out that, in aortic aneurism, the accentuation of the second sound corroborated the modern view as to its production.

*Dr Paton*, in reply, reiterated his views, and expressed his unshaken belief in their correctness.

# OBSTETRICAL SOCIETY OF EDINBURGH.

## SESSION XXXVI.—MEETING VI.

*Wednesday, 14th February* 1877.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Rattray* showed a LEAD WEIGHT which a little boy had swallowed on the 24th January last. It passed per anum on the 27th. No bad symptoms had occurred.

*Dr Wilson* mentioned a case in which a breast-pin had been swallowed and passed without producing injurious effects.

*Dr Cuthbert* alluded to an interesting case in which a boy swallowed a halfpenny, which Mr Annandale had removed three or four weeks after from the œsophagus.

*Dr Gordon* referred to a case in which a set of false teeth had been swallowed.

II. *Dr Young* showed a METALLIC PORTION OF A CHILD'S TOY which had been swallowed and subsequently passed per anum.

III. *Mr H. Stanley Nelson* communicated through Dr Underhill a paper on A CASE OF EMPHYSEMA DURING LABOUR, SHORT FORCEPS, RUPTURED PERINEUM, and OPERATION.

*Professor Simpson* stated that cases of emphysema were very rare, and it was therefore important to have them all recorded. It was interesting to determine in these cases if there existed any pathological predisposing cause to account for the emphysema.

*Dr Underhill* had seen this case both before and after labour, and there was no condition, so far as he knew, predisposing to emphysema. Some dyspnœa was present at the time, but this disappeared after labour was over.

IV. The following NOTE ON A PECULIAR FORM OF VAGINAL HERNIA, by *Dr Matthews Duncan*, was read by *Dr Underhill*:—

Mrs G., who occupied a bed in my ward in the Royal Infirmary, had an ovarian cyst which had long been burst, and had discharged so copious an amount of very viscid clear jelly as to distend her abdomen extremely. An attempt had been made to draw it off, but in vain, on account of its viscidity. In the latter weeks of her life a rounded firm tumour protruded partially from the vagina, sometimes more, sometimes less. It was regarded by the nurse as a falling of the womb. In consequence of her great general illness, and there being no complaint of this tumour, it was not carefully examined during life. At the post-mortem investigation made by Dr Wyllie it was found to be a hernia of Douglas's space. At the bottom of the recto-vaginal pouch was an opening admitting two fingers, which established communication with the hernial sac between the rectum and vagina, and protruding into the latter. The sac was larger than a hen's egg, or about equal to a common apple. It was full of dense jelly, which adhered to its peritoneal surface.

*Professor Simpson* had a patient, at present in the Infirmary, suffering from uterine tumour with ascites, in whom there was a bulging into the vagina, in which the fluid could be felt.

V. NOTE OF CASE OF PAINLESS SECOND STAGE OF LABOUR in a primipara, by Dr Underhill, which appears at page 998 of this month's Journal.

*Dr James Carmichael* mentioned a case of completely painless second stage in a multiparous weakly patient who was the subject of heart disease. The contractions were very powerful, but the patient was not conscious of any pain.

*Dr Rattray* generally found that patients during labour suffered less in the second than in the first stage. Pain was saved by keeping the membranes entire as long as possible.

*Dr Gordon* also thought the first stage the more painful; the second was rather an expulsive effort.

*Dr Simpson* considered that, in the first stage, the pain was more of a worrying nature, and the patient, being less conscious of progress, complained more; in the second stage, the patient, being more conscious of progress, tolerated the pain better. He thought that in these cases of painless labour there was probably some peculiar condition of the sensory nervous system. It was still an undecided question as to the exact site of the pain. Madame Bowin considered it was in the neck of the womb; most observers now thought it was in the uterus itself. He had met with a case in which the labour was entirely painless; the woman was supposed to be the subject of ovarian dropsy. On examination, a bag of membranes was found protruding, and painless labour in progress. Professor von Ritgen of Giessen, in conversing with him on this subject, had mentioned that he had met with seventeen cases of painless labour.

*Dr Young* expressed the opinion that some patients were much more tolerant of pain than others.

*Dr Wilson* had never met with a case of painless labour. He could not agree with Dr Gordon and others that the second stage was so painless as they had stated.

*Dr Underhill*, in replying, stated that he was at a loss to account for the absence of pain in the second stage in this case, as, during the first stage the suffering had been acute. He could not agree with Dr Gordon and other Fellows as to the frequent painlessness of the second stage. With reference to what the President had stated as to the probability of there being some peculiar condition of the nervous system in these cases, he might mention that, after labour, this patient had had a peculiar fainting fit. On one occasion he had attended a lady who was suffering from diphtheria, and in whose case it was necessary to perform tracheotomy; the labour came on soon after the operation and was almost or altogether painless.

## SESSION XXXVI.—MEETING VII.

*Wednesday, 28th February* 1877.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Craig* showed an ENTIRE OVUM, about a fortnight old, which he had found in a blood clot from the vagina, in a case of abortion. Professor Turner had examined the preparation, and pronounced it to be an ovum probably about twelve days old. The preparation was preserved in Müller's fluid.

II. *Dr Macdonald* read a paper entitled NOTE ON THE CONDITION OF CERVIX UTERI IN THE LATTER MONTHS OF UTERO-GESTATION, with specimens, which appeared in the April number of this journal, page 869.

*Dr Gordon* considered that the condition of the parts in cases of placenta prævia went to prove that the cervix gradually disappeared up to the termination of pregnancy.

*Dr Underhill* thought that the Society was indebted to Dr Macdonald for his interesting communication. He thought that the preparation shown by Dr Macdonald proved the correctness of the views advanced in the paper with reference to the condition of the cervix in the latter months of pregnancy. There could be no doubt, he thought, that it was now conclusively proved that the cervix was not taken up to form part of the uterine cavity during the latter months of pregnancy. His experience, therefore, was directly the reverse of Dr Gordon's. Having lately examined several women in the last month of pregnancy, he had found the cervix not at all shortened, and retaining its individuality as distinct from the body of the uterus. In multiparous women he had always found the internal os uteri more patent than in primiparæ. It was practically easy in examining a patient to distinguish between the cervix and the uterus itself, the former having somewhat the feeling of a piece of wet wash-leather; the latter being thick and hard. He had lately seen a case of labour in which the internal os was just beginning to open, yet the cervix was about an inch in length. Delivery was effected about fourteen hours after he had first seen the patient. In this case the cervix had not begun to open up till twenty-four hours before the child was born. The function of the cervix being to transmit the fœtus, was totally different from that of the uterus. He thought, in regard to placenta prævia, that such cases, instead of being considered as opposed to, must be held as proving the point at issue, for, if the old views were correct, every woman with placenta prævia *must* begin to flood as soon as the cervix began to be taken up to assist in forming the uterine cavity, and must continue to flood until she miscarried or died; whereas this was not the history of cases of placenta prævia; and some occurred in which there was no bleeding at all until the woman was in labour at the full time.

*Dr Bruce* mentioned that he had lately had a case of placenta prævia with no bleeding.

*Dr Craig* found that in those cases in which the cervix remained developed up to the period of labour the process of parturition was more tedious than in those cases where it had been incorporated with the uterus.

*Dr Macrae* thought that this case of Dr Macdonald's did not prove the rule, but was quite an exceptional one. He was of opinion that at full term the canal of the cervix became drawn up and incorporated with the uterus, so that, on examination at that time, the fingers simply passed through a foramen, not into a canal.

*Professor Simpson* thought the discussion elicited by the paper proved the interest taken in the question, although some confusion had arisen from the unfamiliar sense in which the word "developed" had been employed in the paper. The preparation shown by Dr Macdonald demonstrated what in mostly all cases we found shortly before labour, viz., a well-defined cervix with distinct canal. We must not, however, conclude from the study of such a preparation that there was no change going on in the cervix during pregnancy from which we might draw conclusions as to the date of gestation. When the cervix in this case had lain beside that in a preparation which he lately showed to the Society, from a patient dying of chorea gravidarum in the fifth month, the canal was seen to be distinctly longer. So it would appear, judging from such cases, that the cervix undergoes a progressive shortening from the fifth month up to the full term of pregnancy, at which period, besides being slightly shorter, it becomes soft and contrasts markedly with the thick and hard uterus. In this way we had generally some guide as to the term of pregnancy. He had found that in multiparæ the canal was generally more open and easily traversed than in primiparæ.

# Part Fourth.

## PERISCOPE.

Gangrenous Tuberculo-ulcerating Syphilid.—*Necker Hospital.*—M. Hardy.—I show you to-day a woman affected with tertiary syphilis, who appears to me to merit very special mention on account of the different phenomena she presents and the reflections suggested by their observation.

This patient is a cook, aged 31. Her constitution is weakly. She has suffered in her childhood from unmistakable scrofulous affections, traces being left of head eruptions, enlarged glands, and cervical abscess. She has had two children, the youngest, who is still alive, is thirteen.

In December last, fifteen days after sexual intercourse, she observed a pimple on the internal surface of the labia majora. Soon this pimple ulcerated, and, a month after its appearance, she began to complain of a general feeling of *malaise*, lassitude, weakness, and want of appetite, and, lastly, such an insupportable headache as for several nights to prevent her sleeping.

Such are, in short, the different symptoms which, more especially in women, often usher in the secondary accidents of syphilis. But the phenomenon to which I would specially direct your attention is this intolerable headache, which is usually considered a neuralgia, though it differs in that the whole head is pained, instead of being unilateral, as in rheumatic neuralgia, and also because it comes on at night with such intensity as to prevent sleep.

After these premonitory symptoms others soon followed, consisting in the development on the vulva, and particularly on the inner surface of the labia majora, of ulcerations which soon extended and formed the starting-point of *plaques muqueuses;* at the same time she noticed the glands of the groin and back of neck grow larger, that they were soft and indolent, and that an eruption of crusts came out on the head.

Then the face, back, chest, and arms became the seat of an extensive eruption of flattened papules, having the nature of a papular syphilid, but with this peculiarity, that some of them presented an elevation of the epidermis, distended with sero-purulent fluid, which in the course of a few days formed a scab. This is the variety of eruption which I have designated a papulo-pustular syphilid.

This patient, who decided to enter the hospital, remained in our wards for about six weeks. During this time nothing special occurred, except the development of a mucous patch on the lower eyelid, a rare position, and a return of the pains in the head without any apparent cause; the headache, which had much diminished, suddenly became so much intensified as to require special treatment. Accordingly for some days this patient took iodide of potassium in doses of two grammes. At the end of forty-eight hours, under the influence of this treatment, the pains had nearly left, and four days later they had completely ceased. In this case the iodide rendered evident service, but only as addressed to the headache, and not to the symptoms proper to the second stage.

This woman left our wards in the end of March. She was then in a very satisfactory condition, to all appearance cured, had regained strength and flesh; nothing remained except the stains of the eruption.

She returned to her work, when, without any known cause, in about fifteen days she felt anew the *malaise* which had already accompanied the symptoms for which she first sought admission, and she soon noticed, in different parts of her body, a new eruption. But this time the eruptive symptoms were of such gravity that the patient did not hesitate immediately to place herself under treatment.

On examination, one is struck by her emaciation and palor, but what is chiefly noticeable is the different appearances of the skin. Lesions of very various characters are to be seen. In the first place are noticed, in certain situations, traces of the old eruption, consisting of pigmentary stains. Then in other places, notably on the chin, entire tubercles are observed, known by their rounded projections, analogous to a splitpea. This is the elementary lesion. Elsewhere, on the arms are seen other tubercles covered with white thick scales, which resemble closely the scales of *psoriasis guttata*, to which the name of syphilitic psoriasis has been given by certain authors—a bad title; for, as is well known, true psoriasis is never syphilitic.

These tubercles are surrounded by a well-marked dark reddish-brown areola, presenting the characteristic tint of syphilitic eruptions.

Besides these lesions we find crusts of a greenish-black colour, well defined, forming a complete circle, quite regular, and composed of concentric layers superimposed one on the top of the other, as is seen in oyster-shells. They result from the drying of the discharge successively poured out, and are thus formed into stratified layers. These crusts are surrounded by a well-marked areola of a dark-brown colour, truly syphilitic.

These two lesions, the squames and crusts, are evolved from the tubercle, which is susceptible of two kinds of alterations. Sometimes the surface of the tubercle desquamates without ulcerating, and thus constitutes the squame; on the other hand, without any apparent cause, it ulcerates and secretes a plastic fluid, which on drying forms a crust. At the same time the ulceration increases in extent, and it is thus that the appearance of concentric layers is explained.

We find, therefore, united in this woman four lesions, of which one, the pigmentary stains, belongs to an eruption already past; but the disease does not stop there. There are seen on the legs, and particularly on the right calf, deep ulcerations, which show considerable loss of substance. They show well the character proper to syphilitic ulcerations, being rounded and excavated; but, besides, they have something special, which is not met with in recent ulcerations. This is a red granulating floor, instead of the grayish floor, due to the presence of a pseudo-membrane which covers syphilitic ulcerations.

One is surprised to see such deep ulcerations coincide with lesions which, on the contrary, appear so superficial, and it would seem to be a true complication. But it is only in their mode of production that these lesions present anything special.

Most commonly the ulcerations are determined by the rupture of little pustules, which, on coalescing, give rise to ulcers more or less extended. At another time ulcerating tubercles are the starting-point of these lesions. Here, on the contrary, they are due to a process of mortification. At certain points in this patient is seen a raising of the epidermis into true bullæ. This skin is converted into an eschar, which in mortifying gives place to an ulceration having no syphilitic characteristic, because it is simply an eschar.

It is thus that this special form of syphilid, described by M. Bazin under the name of gangrenous tuberculo-ulcerating syphilid, is produced.

This form is a very grave one, for the ulcerations which it causes may attain sometimes to a considerable size. In this case the ulcer goes on continuously enlarging; round about it new bullæ are formed, which in bursting increase the area.

To explain this gangrene of the skin in the vicinity of these lesions, there must be an obliteration of the capillary vessels, which causes an obstruction to the peripheral circulation. In this woman is observed, round certain ulcerated patches, a species of purpura, which shows that at these points the capillary circulation is stopped. We have, besides, a microscopical preparation of a tubercle of the skin, showing that the arteries are affected with endo-arteritis; this explains how, in certain cases, they may become completely obliterated and produce as a result gangrene of the skin.

(*To be concluded.*)

SO-CALLED SEPTIC AGENTS IN SITUATIONS INACCESSIBLE TO AIR.—MM. Bilroth and Ehrlich offer some remarks upon the occurrence and development of coccos vegetations in *completely closed purulent collections* (*Centralblatt f. Chir.* No. 2, 1877). The first group of cases relates to cocco-bacterian vegetations occurring in *primary* subcutaneous inflammatory collections. Bilroth has already recorded two such cases. Two fresh ones are, a prepatellar abscess in a woman æt. 59, and a similar abscess in a woman æt. 23. Both cases arose from exertion in washing a floor. In the laudable pus of the first case, there was a considerable quantity of quiescent (ruhenden) micro-coccos chains, and in the pus, mixed with coagulated blood, yet not particularly offensive, of the second case, there were large masses of the same organisms. The last case died of septic poisoning, without any extension of the local disease. Bilroth assumes for such cases that coccos material capable of growth (vegetations-fähige coccos keime) existed in the body, and developed itself in the inflammatory products arising from injury, as the pathological conditions necessary to such development were peculiarly favourable.

The second group includes cases of cocco-bacterian vegetation in closed inflammatory collections in which there exists a connexion by continuity of tissue with open wounds. Bilroth has already related a series of such cases, but the following is new:—"In a man 23 years old, a completely closed fluctuating swelling formed in the left half of the scrotum three days after an operation for left inguinal hernia. Two days later, a clear yellow, odourless fluid, mixed with a little pus, was drawn off through a fine trochar, and was found to contain a limited quantity of free, medium-sized cocca and strepto-cocca, which were powerfully propagable (infections fähig). In all these cases the fluid was odourless. We may assume that in such instances organisms in the primary wounds pass into the cellular tissue and lymph glands by way of the lymph current; but we cannot altogether away with the view that an irritative *inanimate* ferment may result from inflammation, and that micro-organisms may then develop quite accidentally (ganz zufällig).

In a third group of cases cocco-bacteria were seen in closed metastatic collections in the living subject. Four weeks after injury to the finger of a young man, the arm became phlegmanous, and the finger gangrenous. A week later an abscess formed in the right nates, and contained a quantity of offensive pus, with many cocca and strepto-cocca. Again, after a bladder operation in a child six years old, fluid collected in the left knee-joint. The odourless serous contents withdrawn by Dieulafoy's syringe contained large quantities of strepto-cocca; puncture on a subsequent occasion evacuated thick, yellow, odourless pus, free from micro-organisms. The patient recovered with a movable joint. Bilroth therefore concludes that organisms may at one time exist in a closed abscess, and again disappear, and maintains that their absence is not positive proof of their never having been present. Successful inoculations were effected on the corneæ of rabbits, with the above scrotal infiltration after herniotomy.

Ehrlich investigated the occurrence of micro-cocca in erysipelatous skin, and in three cases with a positive result. These organisms were seen in and around capillary bloodvessels, in the lymphatics, and in the interstices of the cellular tissues of the skin.

As regards the *relation* of bacteria *to pyæmia*, Bilroth inclines to the view, that they may play an important part in rendering secretions infections for accidental or intentional inoculation, but that they are not an essential accompaniment of the disease itself.

STRUCK BY LIGHTNING.—A labourer returning home during a thunderstorm, conducted lightning through the points of a pitchfork carried over his shoulder, on to his back, calves, and feet, whereby the soles of the latter appeared to be most burned, on account of his boots being hob-nailed. The clothes and boots were torn to shreds, two-thirds of his back and his fundament scorched, and partly denuded of skin. On one foot there was a flesh wound two centimetres in depth. Under ordinary treatment the patient quite recovered in about three weeks (*Centralbt. f. Chir.* No. 3, 1877).

THE SPREAD OF CANCER.—Colomiatti has examined twenty-two cases of *carcinoma uteri*, and finds that in only five of these the cancer *did not* spread along the sympathetic; in none were the spinal nerves running through the pelvis involved; in only two were large venous trunks perforated by the new growth; and in both of these there were some deposits in the lungs, and cancer-cells in clots taken from the pulmonary veins and right auricle. In every instance the cancer had extended farther along the lymphatics than the nerves. If the ganglia of the solar plexus are degenerated, cancer-cells may either pass along the course of the utero-ovarian and renal, or of the hypogastric and lumbo-aortic plexus. As has already been stated, the *spinal* branches of the hypogastric plexus were never affected. When spinal nerves are actually diseased, it is not likely that cancer-cells have *passed along them*, but it is more probable that the nerve-trunks have been directly invaded by the disease after implication of the surrounding textures. Cancer-cells prefer to creep along the lymphatic space of the perineurium, which surrounds the bundles of nerve-fibrils. On transverse section, one sees the nerve-bundles in the centre surrounded by a thin layer of perineurium, then a quantity of cancer-cells, and finally, most externally, the principal part of the perineurium. In the ganglia, too, the cells pass along the lymph canals. In facial cancer, likewise, the author has demonstrated extension along the nerves. He concludes by laying it down as an axiom, that one of the means by which cancer spreads is the lymphatic system of nerves, quite independently of the other nervous elements (*Centralb. f. Chir.* No. 3, 1877).

In the *Gazette des Hôpitaux* for October last, there is reported a most interesting case of Stricture of the Œsophagus, for which the operation of gastrostomy was performed successfully. The operation was performed by M. Verneuil on the 26th July; and nearly two months after the patient was reported going about, well, and, in fact, gaining weight. The stricture was caused by caustic potash, which was swallowed on the 4th February; from which time up to the date of the operation, the patient, a youth of seventeen, seems to have been almost constantly on the verge of death by starvation. One very interesting and instructive incident in the history of the case was, that M. Verneuil, suspecting that spasm had something to do with the stricture, administered an enema, with chloral, and when the patient was under the influence of the narcotic, succeeded in passing a bougie into the stomach. This was followed by an improvement in the state of the stricture, and the patient was able to swallow a little soup, etc. But in a fortnight the stricture was as impervious as ever both to food and to bougies.

The operation, which was performed with all antiseptic precautions, consisted in carefully cutting through the abdominal parietes on the left side of the mesial line till the peritoneal covering of the stomach was seen, securing a portion of its wall by transfixing it with two long needles, stitching this portion to the left of the external wound, making an incision into the stomach, and inserting a piece of indiarubber tubing. After about six hours milk was injected into the stomach. There were efforts at vomiting, but no regurgitation. There was no peritonitis, no fever, and wonderfully little pain. The antiseptic treatment of the wound was attempted to be kept up by frequently spraying it. The carbolic acid, however seems to have been rather freely used, as there was smokiness of the urine, and slight cauterization of part of the wound. The operation seems to have been accomplished without difficulty, and this, along with the remarkably successful result, is a great encouragement to surgeons. It is to be hoped that the patient will be both able and willing to submit to physiological investigations, which would doubtless be interesting and valuable, as a similar case of direct opening into the stomach has not occurred since that of Alexis St Martin.

## INCOMPATIBILITY AS APPLIED TO MEDICINE.

By Francis Moinet, M.D., F.R.C.P.E.

No doubt all are aware what the word means in common parlance, viz., opposed to, or incapable of combined or harmonious action. In chemistry the meaning is somewhat similar when the term is applied to two or more substances which are not capable of being united in solution without liability to decomposition or other chemical change, very often with the result that a precipitate is thrown down.

While, then, this is what is generally meant by incompatibility in everyday language and as used in chemistry, I wish to have it remembered that it is very often wrong when applied to medicine. We must look beyond the bottle, and consider if the mixture is incompatible with the known action of the ingredients; and if the mixture is not incompatible with these it is not incompatible at all in a therapeutical sense, which is the only incompatibility that we have to consider.

The reason why chemical does not necessarily imply therapeutical incompatibility, is that we must take into account the solvent power of the gastric and other intestinal juices acting under the influence of vital force. So that, while in chemistry the term has a definite meaning about which there cannot reasonably be a difference of opinion, when applied to medicine the case is different, as it is not then simply a question of chemical ethics; it is not only the condition as to the solubility or insolubility of a medicine that has to be considered, but the effect on its activity, that is, its action on the animal economy. Hence the reason why the chemical definition is too limited. It may be, and is correct in some instances, that the condition interferes with the action of the prescription, but not in all, and even when it does, it is, as it were, only by chance, as the definition does not imply it, nor does it logically follow. Neither can we expect it, as the definition was made without any reference to its application to medicinal compounds as affecting their activity. This is the reason why at the present day in prescription writing chemical incompatibility is so frequently disregarded, and must be; and if we wish for examples we have them in plenty in the Pharmacopœia itself. So that some prescriptions and preparations are framed designedly to take advantage of chemical incompatibility for the advantage of the patients. Hence, before condemning a mixture whose components are chemically incompatible, we must ask ourselves the following questions:—

1. Whether the new product although insoluble in the phial may not be soluble in the gastric juices?

2. Whether, as a result of a change, the new compound or compounds be equally active as a medicine?

3. If, in a case of decomposition, the products of exchange are equal in energy to the original substances?

As a general rule, however, we must carefully avoid the combination of substances which may produce unknown or ill-defined compounds, or compounds different from those intended, or whose action is different; hence a knowledge of their composition and action must in every case direct us.

But from what I have said it must be not for a moment inferred that I wish to underrate the importance of chemical incompatibility. In point of fact, that we should neither underrate, nor overrate, but justly appreciate its significance; and a moderate amount of chemical knowledge is quite sufficient to enable us to do so, and prevent us falling into either extreme of avoiding all chemical incompatibles, or disregarding all; because in the former case we should greatly limit our resources in the treatment of disease, while in the latter we might be giving useless or even dangerous compounds.

And while the older practitioners attached too much importance to this form of incompatibility, the new school is just a little inclined to rush into the other extreme. This, I need hardly repeat, is an error in the other direction, and to be most carefully avoided, as it is one fraught with danger to the patient.

Again, in other cases, although the effects may not be dangerous the patient may be taking some compound different from what was intended, or an inert preparation, and so if it does no bodily injury, at least it is wasting time and money, which are important aids to recovery, and at the same time exposing the ignorance of the physician.

And the only thing to prevent us going to either extreme is a knowledge of the proper meaning of the term, based upon a knowledge of those branches of study, viz., chemistry, materia medica, and therapeutics, which alone form the proper groundwork for the rational prescribing or dispensing of medicines.

The next question is, then, Can the action of remedies be interfered with when in combination in any other way than by what is due to opposing chemical properties? If not, chemical incompatibility when it defeats the purpose of a prescription would be the only form.

This, however, is not the case. So that we must, to understand the terms properly, see what a definition ought to embrace if it is a complete one and logically correct.

To do this we must look beyond the prescription, and see for what purposes it has been written,—that is, its action. And have two questions to answer instead of one, viz., If it is an instance of chemical incompatibility, is it also one of medicinal, or, as it is sometimes called, physiological or therapeutical incompatibility? If not, it cannot be fairly condemned, although in some cases, without any sacrifice of its utility, the chemical incompatibility might easily have been avoided.

But although the ingredients of a mixture may not be chemically incompatible, yet their physical properties may be such as to interfere or defeat the intended operation, and that without any chemical change taking place. So that not only the chemical but also the physical properties of certain substances may be a bar to their action. For these reasons a prescription or preparation may be an example of a form of incompatibility which may be termed pharmaceutical in contradistinction to chemical, and be due to the physical properties of the substances used.

But even supposing a prescription complies with the rules of chemical and pharmaceutical compatibility, there is another question which occasionally must be considered owing to the advance of our knowledge of the action of medicines, viz., their antagonistic action, which is so well marked, for instance, in the case of the action of atropine, the active principle of belladonna, and physostigmine, the active principle of Calabar bean.

And what is seen to take place in regard to their actions on the pupil takes place to a certain extent in regard to their action on other parts of the nervous system.

This antagonistic action is not due, remember, to any chemical change, but to their actions on certain parts of the nervous system being of an opposite nature, and so neutralizing each other; that is, what are termed the dynamical or vital properties of remedies, as distinct from their chemical or physical, are incompatible.

This, then, is an incompatibility of a third kind, which, although not so frequently met with as the others, must receive its due share of attention if we are to look at the subject in its completeness.

So that all rules apparently must have exceptions. But looking at the subject as we have done, thus briefly, it will be understood that incompatibility may be of three kinds when applied to medicines, viz:—

1. Pharmaceutical.
2. Chemical.
3. Physiological, therapeutical or medicinal.

The first by reason of their physical properties, the second by reason of their chemical properties, and the third by reason of their dynamical or vital properties interfering with the action by which a prescription or preparation might be curative or salutary, its activity being lessened, increased, perverted, or entirely annulled.

Or we may put it another way, namely, that the action of a prescription may be interfered with by pharmaceutical, chemical, or physiological incompatibility,—all of which propositions are included in the following definition :—

Not suitable to be prescribed together on account of their activity being interfered with by their *physical*, chemical, or dynamical properties.—*Pharmaceutical Journal,* Feb. 17, 1877.

## MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris, and of the Pharmaceutical Society of St Petersburgh; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

SALICYLIC ACID IN SCARLET FEVER.—Dr David J. Brakenridge was led to administer salicylic acid in scarlet fever on account of its antiseptic, antipyretic, and antirheumatic properties, and having read of the success which had attended the treatment of scarlet fever with salicylic acid by Dr F. Weber, Dr Julius Steinitz, and others.

The following is an analysis of nine cases so treated :—In some of the milder cases only one dose, of from 30 to 60 grains, was given in the twenty-four hours; in the severe cases two or three such doses were sometimes given in the same period. Two of the patients were males; seven females. Their average age was 21·6 years. The average duration of the disease before the treatment was commenced was 3·1 days. Dating from the outset of the disease, convalescence, with normal temperature, commenced, in one case on the fifth day, one on the sixth, one on the ninth, one on the tenth, two on the fifteenth, one on the twentieth, and one on the twenty-seventh. In only two very mild cases did the disease run its course without any complication. Albuminuria occurred in five of the seven remaining cases; in two it was slight; in three severe; and in two of the latter it was very persistent. In one of these last two cases the albumen appeared and disappeared at first with the use and discontinuance of the drug. In all of the five cases the albuminuria came on after the medicine had been commenced, and salicylic acid had been detected in the urine. In five cases there was marked delirium, which, in each case, followed the administration of the salicylate of soda, and ceased when its effects passed off. In four cases rheumatism occurred. It is remarkable that, whereas in one case the rheumatism had been present for some days before the salicylate was administered, and was evidently benefited by the drug, in the remaining cases it was not present when the medicine was commenced, but came on during and notwithstanding its administration. In two cases severe pharyngitis and tonsillitis occurred. Epistaxis came on the day after the treatment was commenced in one case. Dropsy also occurred in one case. Profuse perspiration followed the administration of the salicylate in every instance. The drug, as a rule, reduced both the temperature and the pulse, but the reduction was very temporary, and the duration of the fever was evidently lengthened rather than shortened by the treatment.—*Med. Times and Gazette*, 2d Dec. 1876.

SALICYLIC ACID IN TYPHOID FEVER.—At a recent meeting of the Biological Society of Paris, M. Robin asserted that, in typhoid fever, salicylic acid diminishes the quantity of urine, and that it causes an increase in the quantity of indican. He affirms that it produces ulceration of the back of the throat and consecutive œdematous inflammation of the larynx. He states that in typhoid fever very dilute solutions of the acid should be given for fear of diminishing the flow of urine. M. Leven affirms that it is powerless in small doses, and that it causes dangerous derangements of the digestive system. M. Lepine finds that it is very slightly excreted in typhoid, and he found with M. Robin that it sometimes produces a buzzing in the ears, not so intense, however, as that caused by quinine.

In the *Lancet*, 4th Nov. 1876, Dr Cavafy records a case of enteric fever in which the value of salicylate of soda as an antipyretic was well illustrated.

SALICYLATE OF SODA IN INTERMITTENT FEVER.—P. K. Pel has employed salicylate of soda in thirteen cases of intermittent fever. Of these only three were cured, in four a transitory improvement was effected, and in the remaining six no benefit was derived from it. The doses were from a drachm to half an ounce, with phosphate or bicarbonate of soda.—*Deutsch. Archiv. f. Klin. Med.*, B. xvii. p. 314.

SALICYLATE OF SODA IN DIABETES.—Ebstein of Göttingen first suggested the use of salicylate of soda in diabetes. Dr Müller-Warneck of Kiel has lately treated two cases with this drug, and draws the following conclusions from his observations. Salicylate of soda can completely remove the symptoms of diabetes mellitus, but its action is not always permanent. The symptoms disappear more rapidly the larger the dose that is administered, and the longer it is continued. In moderate daily doses (9 to 10 grammes), its initial influence on the diabetic process appears to become gradually exhausted, whereas large daily doses (14 to 16 grammes) exert an increasingly powerful effect on it. The drug may be administered in large daily doses for a long period without any special disturbance of the general health; but any symptoms of poisoning which may appear rapidly subside on discontinuing its administration. Salicylate of soda but slightly irritates the kidneys in diabetes even after prolonged use.—*Berliner Klin. Wochenschrift*, Nos. 3 and 4, 1877.

SALICYLATE OF SODA IN GOUT, ETC.—Dr C. Kunge recommends salicylate of soda for the rapid relief of pain in gout. In two cases a single dose of one drachm was followed in three hours by complete cessation of pain.—*Deutsche Zeitschr. fur prakt. Med.*, No. 28.

Dr L. Hoffman also found it very efficacious in gout of the hands and feet, and he has used it successfully in sciatica, tic doloureux, and intercostal neuralgia. He gives 7½ grains in a gelatine capsule every hour.—*Berliner Klin. Wochenschrift*, No. 34.

Dr Bade states that he has found that salicylate of soda will relieve the pain of mastitis.—*Allgemeine Med. Central-Zeitung*, No. 61.

SALICYLATE OF IRON AS AN ASTRINGENT ANTISEPTIC.—In the February number of this Journal, Mr Robert Kirk calls attention to the value of salicylate of iron as an astringent antiseptic. On adding salicylate of soda to a saturated solution of sulphate of iron, a solution results, consisting of salicylate of iron and sulphate of soda; it is of a bright claret colour, odourless, and with hardly any irritant properties. The cases in which it was applied were generally open sores with an unhealthy action. Progress towards recovery was very rapid after the first few days.

NAUSEA FROM SALICINE: ADMINISTRATION OF THE ACID.—In the *Lancet*, 20th January, Mr Erskine Stuart writes corroborating Mr Sircar's observation as to the nauseating effects of salicin. Ten grains of the salicin induced considerable sickness, while as much as two scruples of salicylic acid produced no gastric disturbance, and no bad symptoms, except the specific catarrhal action on the throat, and constipation. He recommends the following formula for the administration of the acid:—Salicylic acid, bicarbonate of potash, of each three drachms, water to six ounces; a tablespoonful to be taken every two hours. It should be freshly prepared, as it speedily becomes putrid if exposed to the air. He has never found any painful condition of the throat to follow the use of the acid administered thus." Mr Stuart adopts Professor Binz's theory as to the mode of action of the salicylates of potash and soda. He believes that when they reach the tissues, the carbonic acid evolved in the tissues lays hold of the base, and sets free the salicylic acid, which thus exerts its antiseptic influence.

EFFECT OF SALICYLIC ACID ON CHILDREN.—The *Medical Times and Gazette*, 13th January, quotes from a recent number of the *Nordiskt Medicinskt Arkiv*, a communication from Professor Abelin relative to the toxic effects produced by salicylic acid in young children. In cases of diarrhœa with offensive stools it was given on a large scale, and, except in modifying its offensive odour in a slight degree, it was not found to exert any beneficial effect. As an antipyretic, it produced a definite effect in a great number of cases, but, when given in a dose large enough to produce a lowering of temperature from 2° to 4°, was badly tolerated, and produced serious symptoms and great depression. In a dose of about 12⅓ grains to 15⅖ grains it acted on an infant at the breast as a violent poison. It has a very irritant action of the mucous membranes of the mouth and pharynx, and prevents the child from swallowing and sucking. It produces rapidly a lowering of more than 5° F., a considerable amount of collapse, irregular respiration, altered skin functions, and a strong fluxion of blood to certain viscera. To sum up, Professor Abelin is of opinion that in young children the use of salicylic acid must be very restricted. In a dose of 12 or 15 grains it acts as a corrosive poison, in smaller quantities it lowers the temperature without acting beneficially on the course and symptoms of the malady.

In the *British Medical Journal*, 30th December, Dr A. M. Weir publishes a case illustrative of the sleeplessness and disturbed state of the nervous system following the prolonged use of salicylic acid.

FORMULÆ FOR ADMINISTRATION OF SALICYLIC ACID. — The following is the formula recommended by Dr George F. Duffey as being palatable, and unlikely to cause the burning sensation in the throat and gastric irritation which often attend the administration of salicylic acid in large doses:—

℞ Acidi salicylici, gr. 120.
Liq. ammon. acetatis, ℥ii.
Aquæ, ℥vi.

Fiat Mist., and give one-eighth part (gr. xv. of salicylic acid) every hour.—*Brit. Med. Jour.*, 4th November 1876.

In the same Journal for 9th December, Dr P. A. Young states that he uses acetate of potash as a solvent for salicylic acid, and that the addition of aqua carui to this solution forms a palatable mixture. Two parts of salicylic acid are freely soluble in water on the addition of three parts of acetate of potash, or four parts of citrate of potash. The solution in acetate of potash does not give off the vapour of acetic acid on boiling, and free salicylic acid may be extracted from it by sulphuric ether, hence probably no decomposition takes place.

TOXICOLOGY OF ARSENIC.—In a paper recently published by M. Rouyer, and quoted in *New Remedies*, November 1876, he states the following as the conclusions to be drawn from his researches:—

I. ARSENIOUS ACID.—A. *Introduced into the Blood.*—1st, Very small doses suffice to cause symptoms of poisoning to appear: 0·0006 gramme to each kilogramme in the weight of the animal. 2d, Grave symptoms of poisoning, and sometimes death, take place when 0·0023 gramme per kilogramme is injected into the blood. 3d, Death is certain when the dose absorbed reaches 0·003 gm. per kilogramme. 4th, Death takes place in the space of from twenty-four to thirty-five hours when 0·0025 gm. are given, and in eight hours when the dose is 0·003 gm. B. *In the Stomach.*—1st, 0·06 gramme of arsenious acid in solution to the kilogramme of the weight of dogs injected into the stomach is enough to cause death in nearly all cases. 2d, The dose of 0·07 gm. to the kilogramme is certain to cause death. 3d, If poisoning supervened only on administration of a stronger dose it was much more rapid, and this being relative to a particular condition in dogs which throws off the poison too quickly. 4th, In poisoning by the average dose of 0·06 gm. death ordinarily takes place at the end of twenty-four hours.

II. ARSENIATE OF SODA.—A. *In the Blood.*—1st, The true poisonous dose is 0·005 gm. to the kilogramme. 2d, Below this dose grave symptoms appear without causing death. 3d, The duration of these symptoms is from twelve to twenty hours. B. *In the Stomach.*—The dose of 0·015 gm. brings on symptoms of poisoning, but does not always cause death, which, when it does, takes place from twenty-four to thirty hours after.

III. ARSENIATE OF POTASH.—A. *In the Blood.*—1st, The poisonous dose is 0·003 gm. per kilogramme. 2d, In this case death supervenes at the end of seven hours. B. *In the Stomach.*—1st, The poisonous dose is 0·03 gm. per kilogramme, and death takes place in from six to seven hours.

*Antidotes of Arsenic.*—(*a.*) Hydrated sesquioxide of iron recently prepared (gelatinous and brown) is an antidote for arsenious acid, but not for arseniate of potash or of soda. (*b.*) At a longer interval than an hour it is useless to attempt recovery from poisoning by arsenic. (*c.*) For arseniate of potash and arseniate of soda the author proposes perchloride of iron in conjunction with magnesia. (*d.*) The mode of administration is the officinal solution of the perchloride of iron, and half an hour after magnesia in the proportion of a drachm to 3¼ fl. oz. of the perchloride. (*e.*) The perchloride of iron and magnesia is also an antidote for arsenious acid. Therefore it is preferable to employ it always in cases of poisoning by arsenic or its compounds. (*f.*) An hour after the administration of the antidote it will always be well to employ a purgative in order to expel the ferrated arseniate which is formed, and as this arseniate is soluble in acids, to avoid acid drinks and lemonades.

M. Marmé found that, without exception, in all his experiments arsenious acid proved more rapidly fatal than arsenic acid. The fact is against Monck and Leyden's view, that arsenious acid in the blood is oxidized to arsenic acid, and that only as such it dissolves the blood-corpuscles and causes fattening of various tissues and organs. It is probable that when arsenic acid is introduced into the blood it is reduced to arsenious acid, and therefore its action appears more slowly.—*Nachrichten von der Königl Gesellschaft der Wissenschaften*, quoted in *Lond. Med. Rec.*, 15th August 1876.

In a report on Medico-legal Returns in the *Indian Medical Gazette*, 1st April 1876, Mr Robert Harvey draws attention to the frequency with which the endo-cardium is congested or ecchymosed after arsenical poisoning; the left ventricle is most frequently affected. The experience of Drs Bonaria and M'Reddie that the ecchymosed spots are most common near, but not on, the fleshy columns. Briend gives the valves and fleshy columns of the most frequent sites.

# Part Fifth.

## MEDICAL NEWS.

Sir Robert Christison has, we regret to learn, had to resign the Chair on which he has shed such lustre for so many years. We hear that Dr T. R. Fraser of Knutsford, Dr W. Craig, and Dr Henry, who has been assistant to Sir Robert for the past five years, and who has done the work of the Chair during his illness, are candidates.

## OBITUARY.

Dr Clarkson Cuthbert.—We regret to intimate the death of Dr Clarkson Cuthbert, which took place at his residence, 12 Lothian Road, Edinburgh, on the 4th inst. Dr Cuthbert, whose constitution was never robust, was, in consequence of exposure by day and night to the cold, wet weather which prevailed at the end of March, suddenly seized with an attack of pleuropneumonia of the left side, and died, in five days' illness, at the comparatively early age of fifty-one years. He was born at Beverley, in Yorkshire, but his boyhood was spent in Dundee, where he served an apprenticeship to a druggist; and on removing to Edinburgh in that capacity, he was enabled to attend the Edinburgh Medical School, and obtained the diploma of the Royal College of Surgeons. About twenty years ago he commenced practice in this city, and a few years thereafter obtained the degree of M.D. from the University of St Andrews. Subsequently he was elected a Fellow of the Royal College of Surgeons, and became a Member of the Medico-Chirurgical and Obstetrical Societies, before which he read several interesting papers, some of them afterwards appearing in our pages.

He was at the time of his death gradually attaining a good practice, and was much esteemed by his patients. He was for many years an elder of the Lothian Road United Presbyterian Congregation. He leaves a widow, but no family. It may be stated that he was a nephew of the late Clarkson Stanfield, the celebrated marine painter.

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WE must not refrain from the expression of our regret at the sudden death of Mr JAMES BAKER, M.B., C.M., L.R.C.S.E., one of the Resident Physicians of the Edinburgh Royal Infirmary. He succumbed on Tuesday the 17th of April to an attack of acute peritonitis which showed itself only on the previous Sunday, and which probably resulted from the perforation of a gastric ulcer, obscure symptoms of which he had manifested for some months. Born in York in 1851, and educated at a school in that city, Mr Baker commenced the study of medicine in Edinburgh in 1872. His University career was not marked by numerous prizes, for such honours were not coveted by him, nor did he attempt to obtain them. He preferred to occupy himself in quiet work, and was ever looked up to by his fellow-students as a man of very exceptional intellectual parts, and of great promise. Graduating in 1876, he became the House-surgeon of Professor Lister's wards during summer, and last winter he was appointed to a Resident Physicianship under Dr Muirhead. Those who saw him in the course of his ward duties well know the high standard of his professional knowledge, and his conscientious and constant care must have met its reward in the universal esteem in which his patients held him. It is but a very short time since an address was presented to him by those students with whom he had come more closely in contact, expressing in no measured terms their admiration of his talents and their appreciation of his painstaking kindness. Professionally Mr Baker devoted himself more to surgery than to medicine, and in the former department, in particular, his opinion carried much weight among his fellow-residents. Perhaps, however, his most brilliant appearances were in connection with medical politics, where his powerful and eloquent speeches won the admiration even of his opponents. He was elected one of the annual Presidents of the Royal Medical Society in December last, and there his courteous and kindly bearing and his polished oratory will not soon be forgotten. As a man he was cultured and refined in feeling, honest in convictions, and resolute in carrying out in practice what he urged unhesitatingly in principle. As a friend he was firm, generous, and true-hearted. His early death will, we are sure, fall as a heavy blow upon very many, and all who knew him will feel that Edinburgh could not have lost a young graduate of more brilliant promise.

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An esteemed correspondent has forwarded the following interesting communication from a professional friend :—

"Mentone, 6*th April* 1877.

"My dear ——,—I have been here now for twelve days, and am so perfectly well that it seems absurd my staying away any longer, but I don't intend showing face at home for at least a fortnight yet. You have no idea what a glorious place this is for one who has been ill. Ever since I came we have had weather equal to that of our finest summer. Though I enjoy a good toast in the sun, I have had to carry an umbrella as a shade the whole day. Sitting in the shade among the orange and lemon groves is a luxury that you may enjoy the whole day, and even in the evening it is not too cool for a walk. The lemon-tree thrives here as well as the orange. There are thousands of acres planted with them, extending from the shore to 1000 feet up the sides of the hills and valleys. The lemon trees are from 12 to 30 feet high, are loaded with fruit just now to an extraordinary degree, and are at the same time in full flower. They yield three or four crops a year; the oranges only one, and that is mainly gathered. The olive is the most extensively grown tree, forming the 'woods' of the district, and at this season looks well and not brown and withered, as I have hitherto seen it in the autumn; farther up the hills, and indeed everywhere, are plenty of fir trees, some of them very like our Scotch fir, and forming a very pleasant shade. There is a profusion of undergrowth in all the woods and up among the corries; its richness would astonish you. There is the myrtle without end; wild thyme and the prickly yellow broom both in full bloom; beautiful white heaths (which with us would be admired in a conservatory), 3 to 5 feet high, loaded with beautiful flowers; mint, and no end of odoriferous plants, many of them in flower; others just budding. The deciduous trees here are as bare as at home, and are in strange contrast with the active vegetation around them. The fig and the vine are just bursting the bud, and will soon give the hill-sides a more clothed appearance. To give you some idea of the climate, the mountains are terraced for vines up as high as 2500 feet, where the fig also thrives. Four days after I got here I walked up to S.ª Agnese, a height of 2400 feet, and as we are at sea level that means something. It took me three hours to go up and two to come down—not a bad day's work—and every day since I have been out the whole day. Yesterday I took the rail to San Remo and had a look at it, as it is one of the favourite winter resorts. I don't think it nearly so sheltered as Mentone; it wants the backing of high hills, and is not nearly so well sheltered by high projecting headlands on either side. Both face the south, but San Remo is decidedly more exposed to the winds, and I noticed that the lemon trees don't thrive so well, which perhaps is the most delicate test of climate here. The day before I had a run to Bordighera, which is between here and San Remo. It is famous for its palm groves. Palm trees, some of them 50 feet high, grow there by thousands, all in the open air and without any protection. The people seem to take more care of these palms than of the lemon and orange trees, these growing quite as open as potatoes in our fields, while the palms are mostly in walled gardens. Bordighera has the monopoly of supplying palm leaves to Rome for Palm Sunday, and has been a palm depôt for, they say, 1000 years, some of the present trees getting the credit of having attained this age. Bordighera and Ventimiglia are places between this and San Remo, but neither of them are places to reside at, and they are certainly unfitted for invalids. As yet I have seen no place to compare with this as a winter residence. I have yet to inspect Nice and Cannes, but I had a general idea of them on my way through here from Marseilles. There is no end of hotels and pensions here; many of them are very good, and not a few to be avoided. I am located at the Hotel des Anglais (Dr J. H. Bennet lives here), and don't think there is a better in Mentone. By-the-by, Bennet's book is remarkably correct and truthful in its description, and I may also say, its praises of Mentone and neighbourhood. We have the sea in front, then the high road, then the rail, and then the hills. In the East Bay, where I am, the road is close on the sea, and the hotels, pensions, and villas—some with, some without, gardens in front—come next; behind them, at 40 to 80 yards, is the rail, and immediately behind it rises the first rank of hills, covered with orange, lemon, olive, fig, vine, and fir trees; behind these a range of from 2000 to 3500 feet, and behind that again still higher mountains (still white with snow) of from 6000 to 8000 feet. The East Bay is the one preferred by Dr Bennet. The West Bay is also very fine. They are separated from each other by a small part of the Old Town. It is more open than the East. The sea in front, then a broad promenade (Promenade de Midi), then the hotels and pensions; immediately behind these the high road or high street, then more hotels, then the rail, and then the hills, but these are farther back; so that the West Bay is more open all around than the East. I have taken a look at most of the hotels and pensions. The good ones in the East Bay are this one, Hotel des Anglais; Grand Hotel à Gare à Vents; Hotel et Pension (St Maria); Hotel de la Paix; Pension Beau Rivage, Hotel Mirabeau, small; Hotel Belle Vue, Hotel d'Italie, too far up the hill slope; Hotel Grand Bretagne, close below hospital.

These last three I would not send an invalid to. On the West Bay most of the hotels have the sea in front and the high road behind, and are apt to be in consequence noisy; very few of them have any garden ground. The good ones are:—Grand Hotel de Mentone, Hotel Victoria, Hotel Westminster, all with bits of garden. The Hotel Bristol, which used to be in vogue, is in the centre of the town behind, and has no garden in front, and should be avoided by invalids. There is another set of hotels at the back of the town at the West Bay, away from the sea, only the rail is either in front or behind them. They are all good, friends of mine having tried them, and are suitable for invalids. They are:—Grand Hotel des isles Britanniques; Grand Hotel du Louvre; Hotel de Turin; Hotel d'Orient. There are comparatively few villas at Mentone to let for visitors, and some of them look very shabby. The invalid is best off in one of the hotels, where he is readily taken 'en pension.' I pay ten francs a day, and am exceedingly comfortable; overlooking the Mediterranean, lie with my window open all night, and am lulled to sleep by the sound of the waves: the moon shining on the sea is glorious, and the stars shine out with a brightness an approach to which only we have in a clear winter night with a hard frost. At San Remo there are not above a dozen hotels (Hotel de Mediterranée, just building, and will be damp for a year), but many more villas than here, and many of them magnificent affairs, and suitable only for the very wealthy. The winter here has been remarkably fine. Those who have been here are all sunburnt, as if they had just came off a summer tour. They all leave by the end of the month. I leave on Monday for Nice, spend a few days there, and then on to Cannes.—I am, my dear sir, yours truly, ——"

## CORRESPONDENCE.

*To the Editor of the Edinburgh Medical Journal.*

15 SHANDWICK PLACE, *April* 1877.

SIR,—I have lately received a circular from our energetic Officer of Health on the subject of infectious diseases. I do not intend entering on a discussion of that circular, but I gladly avail myself of it as a basis for two suggestions which I have long thought over and desired to place before the profession.

First, I would venture to suggest to our Sanitary Committee and Officer of Health, that a most necessary precaution against the spread of typhoid fever by milk would be the inspection of the cisterns of the dairies throughout the town. These shops are usually in low flats and other inferior places where the proper arrangements for disconnecting the cisterns from the drains are not likely to be perfect. It seems to me, therefore, that consumers of milk run a very great risk, should a case of typhoid fever occur in or near any such dairy. *Verb. sap.*

In the second place, I would suggest that there should be a thorough medical supervision and inspection of our schools. Every one knows that to send a child to school is to introduce him at once to a series of illnesses from infectious and preventible diseases. It seems to me that this ought not to be, inasmuch as proper measures should prevent the possibility of such infection, at least, as far as human means can go. And I venture to point out one source of infection in schools (the greatest source, I believe), which could and therefore ought to be removed. I allude to the practice of parents sending their children to school when there is a case of fever in their house. I think it should be made as much an offence to send a child to school out of an infected house, as for a person to enter a public conveyance with an infectious disease on him. Some parents, of course, will look on such a restriction as obliging them to keep all their children at home, when one is ill, as a great hardship. But it seems to me a less hardship than to compel another parent to send his children to school under the moral certainty that these children will sooner or later be infected with measles, hooping-cough, scarlet fever, etc.

I ask myself, Is Government justified in obliging me to expose my children to infection? And seeing that the State forces me to send my children to school, is it not bound to do its utmost to remove all risk of infection from these places?

Now there are two passages in Dr Littlejohn's circular which give me hope, and have encouraged me to pen this letter. They are as follows:—"I think the state of public opinion on the subject is such, that even compulsory legislation would be, if not welcomed, at least received with satisfaction." And again—"There can be no doubt that, as diseases are allowed to spread unchecked, they entail a larger mortality and heavier pecuniary losses. The time has come when Medicine should more actively practise prevention . . . . and when the practitioner should ask himself, not merely the question, 'How best can I effect a cure?' but the equally important one, 'What means can be adopted to protect the family and the community at large from the spread of infection?'"—Yours, etc.,

A. G. MILLER.

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ROBERT BENTLEY, F.L.S., and HENRY TRIMEN, M.B., F.L.S.—Medicinal Plants. Part 19. London, 1877.

J. M. CHARCOT.—Diseases of the Nervous System. London, 1877. New Sydenham Society.

JOHN COCKLE, A.M., M.D.,—Surgical Treatment of Aortic Aneurism. London, 1877.

D. CULLEN, M.D.,—Some undetermined Points in Typhoid Fever. London, 1877.

WM. HENRY DAY, M.D.,—Headaches: their Nature, Causes, and Treatment. London, 1877.

J. DELAUNAY, docteur de la Faculté de Paris.—Etude sur le Cloisonnement Transversal du Vagin, complet et incomplet, d'origine congénitale. Pp. 130. Prix, 3 fr. Paris, 1877.

THOMAS GURNEY, M.D.,—Cancer, Tumours, and Minor Operations. London, 1877.

E. F. HOSTEING.—Essai sur la Syncope: Recherches Cliniques et Expérimentales. Pp. 93. Prix, 2 fr. 50 c. Paris, 1877.

JONATHAN HUTCHINSON, F.R.C.S.,—Illustrations of Clinical Surgery. Fasc. 7. London, 1877.

ALBERT HYDENREICH, docteur de la Faculté de Paris.—Des Fractures de l'extrémité supérieure du Tibia. Avec 2 planches. Pp. 128. Prix, 3 fr. Paris, 1877.

Henry Lawson, M.D.,—Sciatica, Lumbago, and Brachialgia. London, 1877.

T. R. Lewis, M.B., and D. D. Cunningham, M.B.,—The "Oriental Sore" as observed in India. Calcutta, 1877.

T. R. Lewis, M.B., and D. D. Cunningham, M.B.,—Leprosy in India. Calcutta, 1877.

James Morton, M.D.,—Treatment of Spina Bifida by a New Method. Glasgow, 1877.

Le Dr Notta, Président de l'Association Médicale du Calvados, Chirurgien de l'Hôpital de Lisieux,—Médecins et Clients. 2^me^ *édition.* Pp. 178. Prix, 2 fr. 50 c. Paris, 1877.

F. Panas, Chirurgien de l'Hôpital Lariboisière,—Leçons sur les Affections de l'Appareil Lacrymal, comprenant la Glande Lacrymale et les voies d'excrétion des larmes. Avec figures dans le texte. Pp. 221. Prix, 5 fr. Paris, 1877.

John Parkin, F.R.C.P.E., F.R.C.S.,—Gout. London, 1877.

Louis Pathalt, D.M. de la Faculté de Paris,—Du Bromure de Camphre [Camphre Monobromé de Wurtz] et de ses Usages Thérapeutiques. Avec 6 figures en bois. 4^me^ *édition.* Pp. 51. Paris, 1877.

Le Dr Pouillet,—La Spermatorrhée. Pp. 259. Paris, 1877.

Ch. Robin, Membre de l'Institut et de l'Académie de Médecine; Professeur d'Histologie à la Faculté de Médecine de Paris,—Traité du Microscope et des Injections: de leur emploi de leurs Applications à l'Anatomie Humaine et Comparée, à la Pathologie Médico-Chirurgicale, à l'Histoire Naturelle Animale et Végétale, et à l'Economie Agricole. *Deuxième édition revue et augmentée: Avec 336 figures intercalées dans le texte et 3 planches gravées.* Pp. 1101. Prix, 20 fr. Paris, 1877.

Henri Roger, Membre de l'Académie de Médecine, etc.,—Recherches Cliniques sur les Maladies de l'Enfance. Tome Premier. Pp. 491. Paris, 1872. [The second volume is announced for October.]

Henry Smith,—Sir W. Fergusson, Bart. London, 1877.

Student,—Science of Living. Lond. 1877.

John Sullivan, M.D.,—Endemic Diseases of Tropical Climates. London, 1877.

T. Pridgin Teale, M.A.,—Dangers to Health in our Own Houses. Lond. 1877.

J. Taft,—A Practical Treatise on Operative Dentistry. London, 1877.

Transactions of the New York Pathological Society, Vol. I. New York, 1876.

Transactions of the Obstetrical Society, Vol. XVIII. London, 1877.

William Turner, M.B.,—Introduction to Human Anatomy. Part II. Edin. 1877.

Rachel Williams and Alice Fisher,—Hints for Hospital Nurses. Edin., 1877.

## PERIODICALS RECEIVED.

American Journal of Obstetrics,—April.
American Practitioner,—February–April.
Analyst,—February, April.
Annales de Gynécologie,—April.
Annales d'Oculistique,—January, February.
Annali Universali di Medicina,—Feb March.
Annali Universali di Medicina e Chirurgia,—February, March.
Archives Générales de Médecine,—March, April.
Archives de Tocologie,—March, April.
Archivio Clinico Italiano,—Jan., Feb.
Berliner Klin. Wochenschrift,—February 26 to April 16.
Births, Deaths, and Marriages, Monthly Return of,—February.
Boston Medical and Surgical Journal,—February 15 to April 5.
British and Foreign Medico-Chirurgical Review,—April.
British Medical Journal,—Feb. 24 to April 21.
Bulletin Général de Thérapeutique,—Feb. 28 to April 15.
Centralblatt für Chirurgie,—February 24 to April 14.
Chicago Journal,—February–April.
Commentario Clinico di Pisa,—Jan. to March.
Detroit Medical Journal,—February–April.
Dublin Journal of Medical Science,—March, April.
France Médicale,—Nos. 1 to 30.
Gazeta Medica da Bahia,—Jan., Feb.
Gazette des Hôpitaux,—Feb. 24 to April 19.
Gazette Hebdomadaire de Médecine,—Feb. 23 to April 6.
Gazette Médicale de Paris,—Feb. 24 to Ap. 14.
Gazette Médicale de Strasbourg,—March 1, April 1.
Glasgow Medical Journal,—January.
Indian Medical Gazette,—February, March.
Journal de Thérapeutique,—Feb. 24 to Ap. 10.
Journal of Anatomy and Physiology,—April.
Journal of Mental Science,—April.
Journal of Psychological Medicine. Part I.
Klinische Monatsbläter für Augenheilkunde,—December 1876, and March and April 1877.
L'Année Médicale,—February, March.
London Medical Record,—March, April.
Medical Press and Circular,—February 28 to April 18.
Medical Times and Gazette,—February 24 to April 21.
Medizinische Jahrbücher,—Heft 1.
New York Medical Journal,—March, April.
Nordiskt Medicinskt Arkiv,—Attonde Bandet–Fjärde Häftet.
Obstetrical Journal,—March, April; American Supplement,—February, March.
Philadelphia Medical and Surgical Reporter,—February 10 to March 17.
Philadelphia Medical Times,—February 17 to March 31.
Practitioner,—March, April.
Progrès Médical,—February 24 to April 14.
Revue de Thérapeutique Médico-Chirúrgicale,—March 1 to April 15.
Sanitary Record (The),—Feb. 23 to April 20
Sanitarian (The),—March, April.
Virchow's Archiv,—February, March.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*William Pulteney Alison: His Work in relation to Harvey: Being the Harveian Discourse to the Edinburgh Society, 12th April* 1877. By ALEXANDER HARVEY, M.D., President of the Harveian Society; Professor of Materia Medica in the University of Aberdeen; and Consulting Physician to the Aberdeen Royal Infirmary.

> "The full interpretation of this great discovery is even now not given. For we are still learning what the blood does in that marvellous circuit which never ends, yet ever recommences."—PARKES, *Harveian Oration*, London, 1876.

> "The fluids of living bodies are liable to movements, and variations of movements, peculiar to their living state, but independent of any impulses which they receive from the solids."—ALISON, *History of Medicine*, in Cyclopædia of Practical Medicine.

GENTLEMEN,—My first duty is to thank you for the high honour you have done me in electing me President of this Society for the year now current. And I thank you very heartily. I owe that honour, I know, to the name I bear—the name of the immortal Harvey—the discoverer of the circulation of the blood. Would that I could say that his blood, or the blood of his family, was the same as my own, ran in my own veins and in the veins of my children. Then, indeed, I should be proud: more proud I might well be than were I the descendant of a prince, and had blood-royal in my body. Peradventure, the Harveys of Kent are of the same stock—of the same kith and kin—with the Harveys of Aberdeen. I do not know. I wish they were. For then, apart from my own personal honour and glory, we might all of us here present claim him, in part at least, for Scotland. Be this as it may, let us not grudge England her claim to him as her son, and one of her sons whose name will endure for ever.

In casting about in my mind for a theme whereon to address you on this festal occasion, the 299th aniversary of Harvey's birth, I have resolved to make Dr Alison the subject of it. I am now aware, indeed, that, eleven years ago, Dr Halliday Douglas, then President of this Society, made him his. I was not aware of it, however, not only till after I had made election of Alison for my hero, but had put together the greater part of what I am about to say to you regarding him. Yet it so happens that I shall not in the least trench on the ground Dr Halliday Douglas then occupied. Alison, too, will well bear a second hearing. He was a man so many-sided as to leave scope and verge enough for two of your orators.

The first heading of this address, as given in to one of your secretaries, was, "Jottings of Alison's Work in relation to Harvey, and to the Progress of Medicine in his own day." I called it "jottings," for of labours so manifold in his own profession as were Alison's, and of writings so copious as his, jottings was the fitting word to use for all that could be said of his work in a short address. But as I proceeded with my task, I found these jottings growing unduly upon me. On putting my sheets together, I found that they would occupy, not three-quarters of an hour only, but several hours, to the utter spoiling of this festival every way.

Your secretary—to my horror at the time—struck out the word *jottings*. I am now, however, well content that he did so, because, restricting myself, as I am obliged to do, to Alison's work in relation to Harvey, and to this alone, I purpose doing that great man all the justice I can. And a worshipper as I have been of him these five and forty years, it is a peculiar satisfaction to me to think that, without straining a single point,—that using only the plain words of truth and soberness,—I can, in the matter of the circulation of the blood, link Alison's name with Harvey's—ay, and in the sense indicated by Dr Halliday Douglas, as a *discoverer*, too. Suffer me this assumption, meanwhile—I hope in the sequel to make it good. I wish, indeed, that, before passing to my proper theme, time would allow me to say something of the man, and of his work generally; but it will not. I had put together some thoughts regarding him which I should like to give expression to. But I must forbear. Yet, I cannot altogether forbear. I must say a word or two of his work generally. The grand old man himself I shall remit to your own recollection of him as he went in and out among you, and as he taught you over the way.

In 1831 there appeared his "Outlines of Physiology;" and, in 1833, his "Outlines of Pathology." These two works may be said to have done for his time what Cullen's "First Lines" and John Hunter's "Treatise on the Blood," etc., did for theirs. Fifty years or thereby had meanwhile elapsed. Some systematic work was needed to gather up into one the fruits that had ripened in the interval. Those "Outlines" supplied the want admirably. Bichat in the early part of the century had laid the foundation of a new school in physiology, and in anatomy also. Cuvier followed; and he reared a grand superstructure in both, in the "comparative" direction; and Majendie and others in France and Germany made substantial contributions to the former. In Pathology, Laennec, Louis, Andral, Broussais, and others, also in France and Germany, had made large additions to our knowledge of diseased actions and morbid structures. In this country, Sir Charles Bell, Sir Benjamin Brodie, Mr Shaw, Mr Mayo, and others, had been busy in the department of Physiology; while Dr Abercrombie, Dr Bright, Dr Blackhall, Dr Bateman, Dr Armstrong, Dr Hastings, Mr Travers, and others, had been equally so in that of Pathology. In Ireland, Dr Cheyne, Dr Percival, Dr Graves, Dr Stokes, and others, had been fruitful workers. The first quarter of the century had altogether added much to our stock of knowledge. But where was it all to be had in a concrete or systematic form, in our own tongue? Virtually, or to any good purpose, nowhere,—nowhere at least for students. In Physiology, indeed, we had Blumenbach's Institutions, and Majendie's work,—both of them translations from the original; and, after a time, Bostock's and Mayo's treatises—valuable in their way, yet deficient in many respects. In Pathology there was really nothing, save Cullen's "First Lines," and his "Nosology,"—then the text-books in our medical schools, and in use at the examinations of the Army Medical Board.

At the time indicated, Alison struck in with his two volumes, giving us in a compact and condensed form a complete and truly scientific summary of what had been done up to the time at which he wrote. Singularly rich they were, and exhaustive in the exhibition they presented of facts ascertained. Nor were they less conspicuous as a studied exposition of the principles underlying the facts—a feature pointedly referred to by him in his prefaces as all-important, yet too much neglected. I was myself a student at the time; and I well remember the intense satisfaction they gave. Students felt that, in view of the pass-examinations before them (less stringent then than now, and having more regard than in these days to the capacity of the human mind), they could breathe more freely; and that, in respect of the grand fundamentals of their profession,—to wit, "the functions of the living human body as performed during health, and as altered by disease," they had got something very reliable and very precious; while, with an imperfect system or science of therapeutics, imperfect still and uncertain, the *Vis Naturæ Medicatrix* was then—as now, in this country, as ever and to this day in China and the East, and in Africa and the islands of the sea—the great safeguard of the community. Would that Alison had given us, as he hoped to do, an exposition of the diseased functions of the living body, "as influenced by remedies;" nor this alone, but also of those diseased functions as righting themselves spontaneously or independently of remedies—that is, of the provisions of nature for the spontaneous cure of diseases, and the processes by which the morbid or abnormal changes, dynamic and organic, revert back of themselves to the normal and the healthy. The great *desideratum* in these days is a system of *Natural Therapeutics*. On this alone can we hope to build any satisfactory system of *applied* therapeutics. We must know what Nature, *unaided*, can do, and what she cannot do, in the cure of disease, before we can rear a system of what I have called applied therapeutics, or draw sound conclusions as to the real action and the intrinsic value of our remedial agents. What we need above all things is, an exposition of the *Vires Naturæ Medicatrices.*

The time at which those two volumes appeared was a remarkable and a memorable one. It was the era of the Reform Bill, and of other great political changes. There was then an extraordinary activity in the world of mind in all departments of thought and action. And those two tiny octavos soon passed into oblivion. No second edition of the Pathology was ever asked for, albeit the Physiology happily reached a third—happily, I say, for it contained a singularly valuable additional chapter—the second, "On the most General Laws of Vital Action." But those two volumes did their appointed work. They were themselves rather too strong meat for most students. They needed close application—some considerable effort of attention—like Butler's Analogy and his "Fifteen Sermons." Yet, as I have said, they did their appointed work. By means of them, and by his oral teaching, Alison gave a fresh impulse to inquiry—scientific and practical. He taught others; and he taught them so as to make them supersede him. Carpenter was Alison's pupil, and (avowedly?) gained his inspiration as a physiologist from him. Sir Thomas Watson was also his pupil. And both these distinguished men, coming after him, yet working contemporaneously with him, supplanted him, or they more immediately do so. Dr Carpenter, acknowledging him as a prince among physiologists, apologizes, in the dedication to him of his great work, "Principles of Human Physiology," if he has ventured to differ from him on some points. And are not the "Outlines of Pathology" reflected in almost every page of Sir Thomas Watson's incomparable Lectures? Let me not be misunderstood here. Not that these Lectures could not have been written without the Pathology. For Sir Thomas, himself a master in Israel, had himself been working independently in the same rich and deep mine. It is simply this, that Watson was manifestly much beholden to Alison. And thus, having in those two works of his served well his day and generation, he gave place to those two eminent men, and to others. He stood aside; while yet he continued to enrich the science of Physiology and Pathology with not a few valuable contributions. So far forgotten he was, while still highly honoured and greatly esteemed. And, in my opinion, he was somewhat unduly forgotten. For both these works of his will well bear a reading, and a fruitful reading, in these our own days. Yet, in these days, students are so jaded with work that they have no time for the reading of other than their class text-books, and the medical journals; and, directly they "pass," there comes to most a reaction from professional reading of the higher sort.

One word more as to Alison's style as an author. It has been spoken of as too condensed, as parenthetical, and even in some degree obscure. His writings require, indeed, as I have said, close attention. And parenthetical they sometimes are. Yet this has to be borne in mind, namely, that he had no time to revise what he wrote. For at least twenty-five years of his life he was always writing, while yet he was constantly occupied with professional and other work, and spending himself, too, in abounding labours of love and duty (self-imposed indeed) among the poor. Altogether, the amount of his writings was—simply immense. Then consider further, what must needs have come upon him daily as a professional man (as the care of all the churches came daily upon St Paul)—namely, a continual breaking in upon his time. All this allowed for, and the amount of reading and research, together with a corresponding amount of thought and reflection, also allowed for, I think it will be granted that his work as an author was admirable. There is a singular directness and precision in his style, a singular richness in his matter. To my own mind, there is even a charm in his style; and did time allow me, I could cite not one, but many expressions, sentences, and whole paragraphs of great beauty, quite as deserving of being taken into the category of household words, and into a collection of "elegant extracts," as those of almost any other of our distinguished writers. Those that are to any extent familiar with his multifarious writings must be struck, I think, with the wealth of his mind, with the great extent of his information, with the firm grasp he took of what he gave his mind to, and with his judicial exposition of it all. Imagination he had in a high degree, a far reaching insight into things; and (as Halliday Douglas remarks) he was at once fearless and logical in his judgments. Moreover, the habit of his mind was no less practical than scientific. There was nothing of the book-worm in him. He did not give us "great heaps of facts" of no use to anybody. Dr Andrew Duncan, jun., and Dr Craigie, his contemporaries, probably knew a great deal more than he did. But it may be doubted whether they had his practical insight, or his great grip of principles.

All this as to Alison I stand by. Of the men of his day between 1825 and 1845 (or 1820 and 1845), he was, I hesitate not to say, *facile princeps*,—the foremost man of his day in Edinburgh; and when I say Edinburgh, I mean in the United Kingdom. And I say this with a full admission of the fact (for such it seems to be) that since then, or before the expiry of that time, London men were coming to the front, and now stand in the front.[1]

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[1] I have long and often thought that the *Sydenham Society* would do a real service to the profession, and especially to the younger members of it, were it to gather up into two or three volumes the more important of Dr Alison's miscellaneous medical writings, reviews, etc. Were it only to serve the purpose to the mind (to use a favourite quotation of his own from Bishop Berkeley) that a certain sort of crop does to the soil,—one raised "not for the sake of the

Pardon me, Gentlemen, these preliminary remarks as to Alison. I feel that I cannot help making them. The number of those that well knew him and his work, as I did, is rapidly lessening. And soon, of those that knew him personally, there will be none left to speak of him. One needed to know him living to appreciate him fully. I had the happiness to make his acquaintance in his best days, and to maintain this acquaintance by correspondence to the very end of his life. And it will ever be a pleasing thought to me that he regarded me as a personal friend. Let me add, in a single word, that I have been led to embrace this opportunity of giving expression to my estimate of him by the reflection that he has not yet received either from the profession or from the public the consideration that is his due.

And now, without further preface, I come to speak of Alison's work in relation to Harvey.

What was it in respect of the Circulation of the Blood that Harvey discovered? The entire circuit of the blood in the system, its course or movement as a whole. Before his time something had come to be known as to parts or portions of this movement. Servetus, Colombo, and Cesalpini, who lived in the sixteenth century, had, it would appear, each of them, tolerably accurate notions of the circuit of the blood through the lungs. They seem also to have known that in these organs the blood is somehow acted on by the air. But beyond this they not only knew nothing, but their ideas were positively erroneous. In the century following, some time about the year 1616, Harvey, by a course, a sustained and prolonged course of careful observation, experiment, and induction, made out the actual movement of the blood throughout the entire organism—through the system as a whole. What before was known of the circulation was at once partial as far as accurate, and erroneous as to the rest, and the main part of it. Harvey made our knowledge of its circuit complete and accurate, and he made it clear to demonstration. As Dr Parkes remarks, he "proved the truth of his doctrine so clearly, and by such able arguments, that even at the present day one wanting to prove the

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fruits, but to be ploughed in as a dressing to the soil," and for the enriching of it,—such a collection would be of great value. And here I would take occasion to remark, as I shall find no other place for it, that in his review (in the *Edinburgh Medical and Surgical Journal*, vol. xxvi.) of Travers on "Constitutional Irritation," we have the substance of his chapter in the "Outlines of Pathology" reproduced in the "Outlines of Pathology and Practice of Medicine," on "Cases of Sudden and Violent Death." The review and chapter together are worth their weight in gold. Thoroughly original in its whole conception and treatment that chapter is, and the review no less. And the study of both, as dealing with the border land between Physiology and Pathology, as treating of what Alison rightly calls "the simplest cases in Pathology," may be said to form the fitting introduction to the study of the more complex cases that constitute DISEASE. How well the late Dr P. M. Latham appreciated the value of that study appears from his inimitable "Lectures on Subjects connected with Clinical Medicine" (1836), pp. 125–7.

circulation of the blood must adopt Harvey's method." One thing only he failed to demonstrate otherwise than inferentially, to wit, the actual passage of the blood through or along the capillaries from the arteries into the veins. But the existence of such minute vessels was then unknown, and the optical instruments then in use were inadequate to exhibit to the eye the motion of the blood in these vessels. He "lived before the days of microscopic observation and fine injections." With what deliberation he carried on his researches appears from the fact, that, although he had made good his theory to his own satisfaction in 1616, or before this time, he continued to pursue his inquiries for twelve years longer before promulgating that theory to the world. A rare forbearance this, as Dr Bostock remarks !—Might he not have feared that some one would anticipate him in his great discovery. But in those days men thought twice before venturing to kick against the pricks. Had he lived in Italy or in Spain, the revelation of his discovery might have brought him to the stake.

Then, as to the power or powers concerned in effecting the movement of the blood, Harvey knew that the main power and the essential power lay in the heart. Probably he knew nothing beyond this,—probably he thought of nothing beyond this. But just as he knew nothing of a system of capillary vessels, so he knew nothing of what goes on in them between the blood and the tissues, and could conceive of no power operating there, either in the vessels themselves or in the changes of which they are the seat, as coming in aid of the heart's action in maintaining the general circulation.

The first additions made after Harvey's time to our knowledge of the circulation was the ocular observation by Malpighi of the passage of the blood from the arteries into the veins. This was about the year 1660 or 1661, or fifty years later than Harvey's discovery was completed. What Malpighi saw was in the web of the frog's foot, in the membranous part of the lungs, and in the mesentery. With his eye, he saw "the arteries rapidly projecting the blood in successive waves towards their extremities,—the veins receiving the blood, and returning it in a uniform stream to their trunks." It would appear, however, that he only saw the mere passage of the blood through the smallest arteries and veins ; and this further, namely, that the pulsation is confined to the arteries, and does not extend to the veins. The rapidity of the blood's motion, and the interlacing of the vessels one with another, scarcely permitted him to detect with the eye the exact course it takes.[1]

Nearly thirty years elapsed before any further advance was made in this direction. About the year 1698, Leuwenhock, with the aid of the microscope, saw the circulation in the capillary vessels as it goes on in the wing of the bat and in the tail of a fish. Since then this phenomenon has been familiar to physiologists—known to them in all its details.

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[1] Bostock's Physiology, 2d ed., vol. i. p. 347.

The next step in advance was the discovery that the smaller arteries—on to the smallest that can be discriminated as such—are possessed of the vital property of *contractility.* The researches that led to this discovery seem to have begun by Haller. They were afterwards more fully prosecuted by John Hunter, by Verchuir, Dr Wilson Philip, Dr Parry, Dr Jones, Dr Thomson, Dr Hastings, and others. The kind of contractility possessed by these vessels was ascertained to be that of slowly responding to a stimulus or an irritant directly applied to them or to their coats—the contraction or narrowing induced being permanent for a time, and then slowly relaxing before the vessels resume their normal calibre. And to this form of contractility the term *tonicity* was applied. It differs widely, as you know, from the contractility exhibited by the heart, in which the process of contraction, rapidly induced, is as rapidly followed by relaxation. Still further: this attribute of tonicity came, subsequently or contemporaneously, to be associated with the possession by the smaller arteries of a coat essentially *muscular* in its structure.

This ascertained, physiologists were naturally led to inquire whether, in virtue of this property inherent in them, the arteries may not come in aid of the action of the heart in carrying on the general circulation. And Haller "adduced a variety of considerations to prove that the contraction of the arteries is an important agent in the circulation of the blood" (Bostock, *ut supra,* vol. i. p. 393). So also did Blumenbach. He could not see how otherwise the circulation could be carried on in the vena portæ or in the placenta.

This question we shall have to consider presently in connexion with Alison's views. But just at present I would take occasion to observe, that in discussing this question, physiologists have not always been sufficiently careful to discriminate between two things,—to wit, on the one hand, the general circulation, and in connexion with this, the general action of the arteries; and, on the other hand, partial or local aberrations in the movement of the blood, and these in connexion with increased or decreased action of the arteries in the parts concerned. And they have not unfrequently imported discussions regarding the one into discussions regarding the other, and in such manner as to breed real confusion. Even Alison, following herein the example of his predecessors, in discussing the question whether any, and, if any, what additional power comes in aid of the heart's action in carrying on the circulation, mixed up the one with the other. Had he avoided, as he easily might, mixing up these two things, he would, I think, have done himself greater justice in his exposition of this department of Physiology, and placed the whole subject on a better because a clearer footing.

To proceed: Dr Alison first shows, as is easily done, that the action of the heart is *essential* to the motion of the blood in all parts of the body. As to this, let it suffice to say, that on the cessation of the heart's action the circulation of the blood comes instantly to a stand everywhere.

He next makes it his business to show that the action of the heart, although essential, is not the sole or the only agent concerned in that movement. He strives to show that, whatever be its nature, or however it may operate, some other power, cause, or condition is equally essential with that of the heart in the maintenance of the general circulation. And he finds the *crowning* proof of it in one single yet decisive fact,—"the striking fact," as he calls it,—namely in this, that when the aeration of the blood in the lungs is suspended, completely and at once arrested, the whole circulation comes almost at once to a standstill,—first in the lungs, and next, and necessarily, in the whole system. He might well have put this fact and proof in the foreground of his argument, instead of leaving it, as he does, to the end of it.

Now, as far as I know or can discover, Dr Alison was the first to adduce this fact in proof of the assumption that there is a power, or that there are powers, equally essential—I do not say *alike* essential, nor does he—but equally essential with the heart in carrying on the circulation. I can see no reference to the fact in question, with that special purpose in view, in any physiological or other medical writer before him. And it is clear that to him the proof was decisive. Attempts have been made, I know, to show that the fact is not of the value he imagined. But, supported as it is by a large body of evidence then and subsequently adduced by him, and collateral evidence, towards the gathering of which he paved the way, it cannot, I am persuaded, be gainsaid. Nothing can more strikingly present the fact to the mind, or the import of it, than a well-known experiment of Bichat's.[1]

This proof, adduced in support of the assumption that there is an agency concerned in maintaining the circulation of the blood distinct from that of the heart, and equally essential thereto as is the action of the heart, and that assumption allowed,—meanwhile, at least, the question arises, What is this agency, what this power, what its nature, what its mode of action?

Dr Alison takes up and pursues this question. The supplementary power concerned obviously resides at a distance from the heart. It clearly lies in, or in the immediate neighbourhood of, the capillary bloodvessels. Is that additional power, then, the vital contractility of the smaller arteries, in *act?* Does it reside in these minute vessels? Is it in any way connected with them and their known property of tonic contractility? Dr Alison maintains, and he demonstrates conclusively, that it has nothing to do with the exercise of that power inherent in these vessels. He shows that, while this power may, and does by its exercise *affect* the motion of the blood in *individual* parts, or locally, it is of such a nature that any *general* exercise of it, that is, that its equable exercise everywhere, or generally throughout the system, could have no other effect than that of retarding, impeding, opposing, instead of promoting, the general movement of the blood onwards. This inference, besides being conclusively established by sundry experiments and several considerations referred to by him, is abundantly clear on a moment's reflection.

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[1] "If a stop-cock be adapted to the trachea, exposed and divided, and afterwards it be shut, the blood becomes darker, and springs for a short time with its usual force" from a cut artery, "but at length the jet becomes gradually feebler. *Readmit* the air, the blood becomes almost immediately red; its saltus also rapidly increases."—*Sur la Vie*, etc., p. 222; quoted from Dr Kay-Shuttleworth's paper in *Edin. Med. and Surg. Journal*, vol. xxix. pp. 41, 42.

In point of fact, it is in one instance only, it is on one occasion only, that this inherent vital contractility of the arteries comes in aid of the action of the heart; and that it does so *then* is a matter of certainty. It is the very last act of that organ, it is in the *articulo mortis.* The heart in this its last act sends out the blood that is in it. The aorta and the larger arteries help also by their elasticity to propel it on. And the smaller arteries everywhere, having now nothing behind them to keep them to their ordinary or wonted calibre, gradually contract upon themselves and squeeze out what of blood remains in them into the veins. This, I repeat, is the only occasion on which, in virtue of their contractility, the smaller arteries can or do come in aid of the action of the heart, in respect of the general circulation. And Dr Cullen, in as far as he understood the property of these minute vessels, made their general action throughout the system (in a morbid sense) the basis of his theory of fever. Calling it a *spasm* of the extreme vessels, this spasm leads to an increased action of the heart in order to the resolution of it. He seems sufficiently to have understood the *necessary* effect of any general exercise of the vital power of these vessels.

What purpose that property of tonic contractility possessed by the smaller arteries serves in the animal economy it does not lie to my hand to consider. Suffice it to say, in relation to Dr Alison's teaching, that it has neither part nor lot in the matter. It has nothing to do in carrying on or in maintaining or sustaining the general circulation. Its general exercise could have but one effect, that of opposing, impeding the action of the heart. Inject an irritant fluid into the aorta of a newly-killed animal, while that property of contractility is still extant in the vessels, the fluid will be arrested in its course.

Nay, more, Dr Alison ends this part of his inquiry by throwing the bloodvessels overboard. As mere canals, indeed, or carriers, they bring the blood into relation with certain other parts than themselves. As such they bring it under the dominion of certain other laws than that of contractility,—laws and parts which he has in his far-seeing eye. They are laws, he adds, by which the movements —observe what he says, the *movements*—of the nourishing fluid are effected (not *affected*, observe, but *effected*) in those animals that have *no* heart,—nay, *no* circulating system,—and—and in *vegetables*. Here, as regards the agency or the power now in view, the bloodvessels are put out of court altogether.

Wherein, then, lies this power, or these powers, that are independent of the bloodvessels, but which come in aid of the action of the heart, and are essential to the maintenance of the circulation in all parts of the living body? What are those laws, under the dominion of which, to use Alison's own words, the blood is thus brought? It is, again, to use his own words, as we have them in the first edition of his "Outlines of Physiology," it is "the *vital affinities*,"—affinities "which regulate the chemical changes that are wrought on the blood in the capillaries." Permit me here to remark that, along with the chemical changes there wrought, Dr Alison might and ought to have coupled the *plastic* changes, more important, agreeably to his own teaching, and of far greater magnitude than the chemical, in the animal body. Perhaps, however, he used the word chemical as embracing the plastic also. But this by the way. It is this power of vital affinity that is, or that furnishes, the additional or supplementary agent in maintaining the general circulation. This conclusion he reaches after sundry considerations which he adduces, and, very particularly, on reflecting on the fact that on "minute examination of the progressive development of parts in the fœtal state of different animals, it appears that various parts are in progress of organization (implying regulated movements in the fluids) before the heart exists, or the arteries are distinctly formed." This argument he brings forward, after having shown the utter insufficiency of the vital contractility of the arteries in the maintenance of the general circulation. And then, having adduced this very powerful, and indeed in itself conclusive argument, he clenches it by a decisive reference to the fact (already adverted to) that "when the chemical changes at the lungs are arrested by obstruction to the access of air thither, the contractions of the right ventricle, although continuing vigorous for a time, fail of their wonted effect, and the blood stagnates in the capillaries of the lungs" (page 43). With nothing directly interfering with it, the right heart fails to drive the blood outwards. It fails because of some cause operating in the capillaries into which it has pumped the blood. This cause is simply the arrest of the chemical changes going on there between the air and the blood. Should this arrest be permanent, the arrest of the circulation there and throughout the system will be permanent, and death will ensue. Yet, readmit the air within a reasonable period, those chemical changes will again be effected, the blood will again move on, and the action of the heart will be resumed.

To that power of vital affinity to which he ascribes the aeration of the blood, he subsequently refers also (in another part of the Outlines) all the phenomena of secretion, excretion, and interstitial or molecular nutrition. These phenomena, he says, and says truly, "do not take place fortuitously or at random, but according to fixed laws," while they "are inexplicable by and inconsistent with any principles that can be deduced from the observation of dead matter, or of other functions of the living body." "We refer them, therefore, to a vital property, our notion of which is, as yet, necessarily vague and imperfect, which modifies chemical affinities in the living body; varying in different parts of the body, and causing these to be differently affected by, and produce different effects on the blood that pervades them, influencing, likewise, no doubt, the chemical nature and relations of the blood itself. To this property the best name that has been given is VITAL AFFINITY. Its existence will, no doubt, always be an ultimate fact in physiology; but the limits of its agency, and the laws according to which it modifies the chemical relations of the substances subjected to it, may no doubt be ascertained, and their development will probably constitute the next great discovery in this science" (Outlines, 1st ed. p. 73).

And so in a manner it did in other hands, his own not wholly excepted. This generalization, however, was, as far as I can make out, Dr Alison's own. And a splendid one, I venture to say, it is. It seems, indeed, so obvious that one may say in this matter, Alison taught us nothing but what we knew before. Why then did not the physiologists that went before him at least refer to it, indicate it, name it? But it is the old story, so pithily put by Archbishop Secker: "Truth *proposed* is much more easily *perceived* than, *without* such proposal, it is *discovered*." Ay, there's the rub. It is easy to see a thing put plainly before us, and yet we might never have found it out for ourselves. Rather should the .exclamation here be, "Dear me, how simple it is! why did I not think of it myself before!" I say that generalization was Alison's own. And Professor Gairdner, I see, in a very concise and beautiful statement of what he calls "Alison's physiological system," in his notice of that great man, credits him with it, sets it entirely to his account.[1] How the idea of that vital principle grew upon him, and with him, as years rolled on! what a flood of facts bearing on it he brought together in subsequent editions of his Physiology, and in his article or chapter on Inflammation, you all know. And you know also how it has received its development in the discovery of cells and cell-growths, and through the prosecution of histological research and of organic chemistry. That the principle in question is a real one, and that it is also a fundamental one in physiology, will not be disputed, except by those that do not go to the root of things in this science,—that occupy themselves rather with special details or departments of it, and have no fancy for what they may regard as abstract speculations,[1] or except by those that discard altogether the notion of vitality. With these last I have nothing to say, being myself a vitalist. But to those that admit vitality, I would humbly venture to submit, that they can no more do without this principle of Vital Affinity in explaining certain phenomena in the animal economy than birds can fly without wings.

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[1] Obituary notice of "the late Dr Alison" in *Edinburgh Medical Journal* for November 1859, pages 475-76. But, indeed, even for that further view of this vital principle, which he speaks of under the head and by the name of Vital Attraction and Repulsion, Dr Alison claims for himself the credit of it. Remarking that "this principle of vital attraction and repulsion is more fully recognised by the physiologists of Germany than by those either of France or of this country," he goes on to say, "as I confidently expect that, notwithstanding the opposition it has lately met with, it will gradually make its way in physiology, I hope I may be excused for observing that almost all the facts stated in this volume in illustration of it were noticed, with the intention of establishing the existence and illustrating the importance of a principle affecting the motion and distribution of the fluids of living animals peculiar to their living state, and independent of the impulse or pressure of their living solids, in my lectures, and in a paper read to the Royal Society here, before the publication of the important works either of Müller or Burdach."—Preface to 3d edition of "Outlines of Physiology," p. v.

This was the point reached, this the doctrine confidently promulgated by Dr Alison in 1831, in reference to the moving powers concerned in carrying on the general circulation. In my opinion he made good the point. He established his assumption that, while the action of the heart is essential to the maintenance of the circulation, there is yet another power in its own way not less essential thereto. He showed that this additional power does not reside in the arteries, and is not the property of contractility inherent in them; nay, that it is wholly independent of that property, and extrinsic of the bloodvessels; and, specifically, that it is the power of vital affinity by which the changes, chemical and plastic, that are constantly going on in the capillaries between the blood and the tissues are effected. Let me just remark here in passing, that this view had already culminated in his own mind in a new theory of Inflammation. This sufficiently appears from his Heads of Lectures on Pathology, appended to the first edition of his Physiology; and in a couple of years thereafter, that theory, the one now received among us, was fully developed in his "Outlines of Pathology." To this I will advert hereafter should time permit.

Meanwhile, suffer me to make a few further remarks on what I may call Alison's supplementary work in relation to Harvey.

Taking his stand on sundry collateral considerations, Dr Alison, as I have said, clenched the proof of his affirmation that the changes going on in the capillaries between the blood and the tissues, and effected there by vital affinity, are equally essential with the heart in carrying on the circulation, by the fact of what obtains in asphyxia. A more decisive actual proof of it he could not, I think, have furnished. His theory of asphyxia, indeed, has, I know, been called in question; but to little purpose, I apprehend. It stands good to this day, and it can be substantiated beyond all cavil or dispute, by additional considerations which Dr Alison might have adduced, but did not otherwise than inferentially.[1]

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[1] As to this, Dr Alison himself well remarks in one of his prefaces: "It seems to me that there is in many medical writings an indifference to the establishment of such general principles, both in physiology and pathology, as the information we already possess sufficiently warrants, which argues inattention to the value and importance of these principles."—Preface to "Outlines of Physiology and Pathology," 1833, pp. x. xi. And, no doubt, in making this remark, he had this principle of vital affinity specially in his eye.

As to this, let me ask you to glance for a moment at what obtains in the capillaries of the general circulating system. Founding on what holds in respect of the pulmonic capillaries in asphyxia, Dr Alison might well have asked, and bade those that took exception to his inference from it make answer,—Supposing it were possible to arrest the processes of molecular nutrition, and of secretion, excretion, exhalation, and absorption in the general system everywhere and simultaneously, as it chances to be possible to arrest the aeration of the blood in the lungs, do you imagine that the general circulation would or could then go on? For this would be the *parallel* case. Circumstances prevent such an observation being made, or an experiment of that kind being tried. But can any one suppose that the result would be different,—that such an arrest would not of itself, and independently of any agency acting on the heart, be followed by an immediate arrest of the circulation throughout the whole organism?

And the inference drawn by Alison from what holds in asphyxia comes to be all the stronger when we think of the simplicity, the absolute simplicity, of the changes effected in the lungs in the process of aeration, as compared with the complexity of the processes of interstitial nutrition and glandular secretion. In the case of the lungs, it is a simple affair of transudation and imbibition. The blood simply gives off carbonic acid; it simply takes in oxygen. There is no complex process of cell-growth as in secretion. There is no such process of disintegration of dead tissue and the reduction of this dead tissue into carbonic acid, urea, etc., as takes place in molecular nutrition. There is no building up anew of new living tissue by existing living tissue, to replace the old and the dead and the cast-off tissue. In the process of aeration there is a simple transudation and exit of carbonic acid, a simple imbibition or sucking-in of oxygen—nothing more.

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[1] Assuming the general principle contended for by Alison to be sound, namely, that the arrest of the circulation in the pulmonic capillaries is primarily and essentially due to the arrest of the changes which naturally go on there between the blood and the air, a further question may be raised and may admit of solution, to wit, How is it that the arrest of the aeration of the blood leads to arrest of the circulation through the capillary vessels of the lungs? The solution of this question, however, would not invalidate Alison's theory. It would only explain or elucidate it. See, as to this, Dr George Johnson's Lumleian Lectures (Lecture I.) in *British Medical Journal* for 14th April 1877.

And yet the arrest of this simple process in the lungs brings the circulation to a standstill there. And it does so in a trice. Contrast this with the complexity of the processes that go on in the capillaries of the general system, go on there everywhere and incessantly, the processes of interstitial nutrition, secretion, and, in the case of the lacteals, of absorption. Can any one challenge the force of this larger yet parallel inference?

As to one of these processes, the most fundamental of all, and that to which, as far as the organic life is concerned, all the other organic processes are subservient, is that of *molecular or interstitial nutrition*. And as to this process I would submit, that neither in relation to the matter now in hand, to wit, the general circulation of the blood and the moving powers concerned in it, nor yet in relation to itself, do our physiologists (the late Professor Hughes Bennett excepted) treat of it in their writings as it deserves to be treated. They refer to it, but they do not analyze it, or put it in detail before their readers, or bring it prominently into view as that which furnishes the *raison d'être* of every other. Still less do they seek to refer it to its fundamental vital principle, that of vital affinity.

As it has a positive, and, indeed, a cardinal bearing on Alison's theory, and is but the analogue of that on which he mainly rested that theory, suffer me in as few words as possible to put the *essentials* of that great process clearly before you.

Long ago, yet not long after Harvey's time, a very distinguished prelate—Bishop Jeremy Taylor—gave expression to a fundamental fact in physiology, anticipating herein both Alison and Carpenter.

"Our very life," Jeremy Taylor says, "is nothing else than a succession of dying; every day and hour wears away part of it; and so far as it is already spent, so far we are already dead and buried."

A great truth this in animal physiology!—the foundation-truth, in fact! And beautifully put it is; expressed it is with a scientific precision wonderful for Jeremy Taylor's time. Yet, what a rare imagination Jeremy had! And our friend Alison herein following up Taylor, and anticipating Carpenter, has said, in regard to it, once and again, and in varying forms of expression: "All vital action is of exceedingly short duration in any living part." This, I say, is the foundation truth in animal physiology—in as far as relates to the "organic" life. No part of the animal body retains its vitality long. The *chemical* affinities by which the various organic compounds are formed, and the *plastic* affinities by which these compounds are transformed into the several organized tissues, and these again knit together and built up into the one whole body or organism, are exceedingly transient in their agency. Every particle, every molecule, every little bit of living matter thus formed within us, retains its vitality but for a brief space only—a space, however, longer with some, shorter with other molecules. Each of these molecules may be said to have its own specific purpose to serve in the economy; and this purpose served, it straightway dies—dying, it may be said, in and by the act. Every muscular effort we make, every exercise of thought and will, exhausts the vitality of so much brain and nerve and muscle; and it does so, it would appear, in the exact measure and proportion of the acts of thought and volition and muscular exertion. Every part has some purpose to serve, some office to fulfil; and the great law is, that every effort and exercise of its function entails the extinction of its vitality, entails its death. We die daily, bit by bit, and thus dying, we are reduced to the condition of dead matter. This dead matter of our bodies is then disintegrated. It is acted on by the oxygen of the air we breathe; and, in the shape of carbonic acid, uric acid, urea, cholesterine, and sundry salts, it is cast out into the draught. There is no play or exercise, indeed, of vital affinity here. It is all an affair of ordinary (inorganic) affinity, albeit going on within the living organism. Nor can we doubt that in its own way and degree it is the source and the occasion of *a part* of the movement of the blood in the capillaries; and if so (curiously enough), it is an exception so far to the general principle so strongly insisted on by Alison, that none of the phenomena peculiar to living beings are explicable by a reference of them to the principles that regulate the changes going on in the inorganic world.[1]

We die daily then; and the extent to which we do so may in a rude way be measured by the amount of the daily excreta, still better by the quantity of food and other aliment we take in daily. Thus thought of, the activity and the magnitude of this fundamental vital process will clearly appear.

We die daily then. Yet, as we continue to live, and to live on, a counterpart process to that indicated by Jeremy Taylor must also day by day go on within us—nay, hour by hour—incessantly. As we die daily, we must needs daily rise to life again. And so we do. The molecules that die inside us and are cast out of us must be replaced by new ones. If every day and hour wear away part of us, every day and hour must renew part of us. And this process of *renaissance* is in fact continually going on within us. And a wonderful process it is, when we come to think of it in detail—living muscle, living nerve, living bone, ere they themselves depart this life, severally building up anew, as by a generative act, from one common pabulum—the blood, new bone, new nerve, new muscle, setting them respectively in their right places, and endowing them with their own proper attributes.

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[1] Those physiologists, however, greatly err that think that because they can form urea in their laboratory, a time may come when they shall be able to form albumen there also; for urea as formed within the living body is a product of organic matter already dead.

A great process this really is, the process continually going on within us, of dying and rising to life again! Little thought of it seems to be by some physiologists, perhaps because it goes on within us silently, insensibly, unconsciously to us, perhaps because it involves merely abstract details.[1]

And a fact it is in relation to the question of the powers concerned in effecting the general circulation, or the movement of the blood in all parts of the body.

The fact, the experiment we may call it, founded on by Alison in his theory as to these moving powers, may be said to be a beautiful as it is a capital one, to wit, that as to the effect on the general circulation of the non-aeration of the blood. It is a beautiful one through special circumstances. The whole blood of the body has of necessity to pass through one single organ in every general circuit it makes; and the function of this one organ is absolutely under our control. We can at once put a stop to it, and then watch the effect of this; and we can set it agoing again, and take note also of the effect of this restoration.

A like experiment we cannot try with the general circulation. Obviously we cannot. Yet, judging from that on the lesser circulation, and keeping in view what has now been said as to the process of molecular nutrition, to say nothing of the complex processes of glandular secretion and lacteal absorption—a continual affair of cell-growth and of much beside—can any one doubt that, *were* it in our power simultaneously to arrest these processes, the entire circulation in the capillary bloodvessels everywhere would not be brought to a standstill, even *superstite actione cordis et arteriarum* for a minute or two?

This wonderful process has well been designated by a French physiologist—the "*turbillon vital*" of the living animal body. Very expressive that word *turbillon* is. What goes on in the ciliary frill of the muscle or oyster as seen under the microscope, the incessant rapid waving of the ciliæ, may give some idea of it. Put a sudden stop to that *turbillon*—can any one suppose that the general circulation would not be at once arrested? If so, should we not have in that arrest the exhibition of a power, not less essential than that of the heart itself, engaged in carrying on, and in maintaining and sustaining the general circulation?

The idea of this, and the demonstration of it, we owe to the genius of William Pulteney Alison. Neither in Blumenbach, nor Majendie, nor Bostock, nor Mayo, is the smallest reference made to this grand motive power.

The power in question, that of vital affinity, was regarded by

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[1] The author hopes he will be excused observing here that the views set forth in the text in regard to this great process, and substantially as there advanced, formed the subject of his inaugural dissertation on Interstitial Nutrition, on occasion of his graduating M.D. at the University of Edinburgh in 1835.

Alison as a moving power. Clearly it was; and as such we have, of course, been considering it here in relation to Harvey's work. In what sense, however, Alison regarded it as such, and how far he afterwards came to associate it with another power allied to vital affinity, yet seemingly in his mind so far at least distinct from it, because named by him separately from it,—namely, that of *vital attractions and repulsions,*—time will not allow me to consider. I content myself with this passing allusion to it.

But to complete Alison's views as to the agency of vital affinity, I must needs refer, however briefly, to another point. To this power of vital affinity he ascribed also all *local* aberrations or deflections of movement in the circulation of the blood, local congestions (normal and abnormal); and the reverse of this, the lessened flow of blood to parts. To him, the theories of increased or decreased action of arteries were of small value, indeed of none. *Ubi stimulus, ibi fluxus,* and the reference of this to vital affinity, its agency entailing movements of the blood this way and that, was simply another expression of his theory.

And his views here culminated, as I have said, in a new theory of *inflammation.* You are yourselves familiar with the old theories of that morbid process—how the main theory turned on the question of increased or decreased action of the bloodvessels. And you will remember, many of you, what debates we *did* have about this in our medical societies—about the question, "What is the proximate cause of inflammation?"

Now, as early as 1831, in his writings, and no doubt earlier still in his teaching over the way, Dr Alison clearly indicated that he saw the insufficiency of either or both of these states as furnishing an explanation of the *essential* nature of inflammation. In his Physiology, as we have just seen, he sets both of them aside as affording any explanation of even normal determinations of blood; and he points to changes going on in the capillaries, but referable to the power of vital affinity, as the main cause or occasion of them. And in the Heads of Lectures on Pathology appended to that work, he says—"As it is very doubtful whether the action of vessels is the sole cause of the motion of blood in the capillaries (independently of the heart's action), so it is equally doubtful whether any alteration in their action is the sole cause of the different phenomena that occur in inflamed parts; and it is certain that the changes wrought on the blood in inflammation cannot be explained merely by alteration of the force compressing and propelling it." The one "very doubtful;" the other "certain!"

This he said in 1831, and in the mere jotting of a lecture. Allow that then, like the man in the Gospel cured of his blindness, who saw men as trees walking, Alison had only caught a glimpse of the truth, one may say of that glimpse, what Cullen says of a single word introduced by Boerhaave (and after the lapse of forty years) into one of his aphorisms—"What a change of doctrine that points at"—*points at.* What I have quoted from Alison points at a real revolution of doctrine: it was a mine sprung on the old theory. Carefully considered, one may see in those few words the actual germ of his theory as afterwards more fully evolved. In 1833, when his "Outlines of Pathology" appeared, he gave that theory to the world in all its fulness, and substantially as he gave it in his latest exposition of inflammation in his "Outlines of Pathology and Practice of Medicine," published in 1844. I have both his versions of it, his earliest in 1833 and his latest in 1844, now lying open before me, and I can see no essential difference between them. With Alison inflammation was, at both times, *a perversion of the process of nutrition,* referable to a perversion of the fundamental vital power of vital affinity.

The late Dr Hughes Bennett, if I mistake not, has put in a claim to something in the discoveries made as to the nature of that morbid process. For my own part I cannot discover wherein it lies. I have carefully compared Dr Bennett's exposition of it with Dr Alison's, and I cannot see wherein they differ. And Dr Alison had given his exposition of it to the profession before Dr Bennett (I presume) began his medical studies. Dr Bennett went perhaps more minutely into certain of the details regarding that process and its products than Alison did, and he used perhaps somewhat different words in his exposition of it. This, however, is a very small affair; and I do not see that Bennett added anything to what Alison had already told us as to its essential nature. In his primary reference of the whole process to an abnormal state of the fundamental vital process of *nutrition,* and this to an aberration of the power of *vital affinity* operating in this process, Dr Alison's theory lay. And Dr Bennett's theory is essentially the same—words perhaps apart. Some have claimed for the Prussians the credit of winning the battle of Waterloo. The Prussians came up indeed in force in time to slay the slain. But the battle was won—the victory achieved—when Wellington's memorable order was given and executed—"Up Guards and at them." How apt we are to be cheated by words! "*Credunt homines rationem suam verbis imperare; sit fit etiam ut verba vim suam super intellectum retorqueant.*" (This admirably expressed truth you will find, as a quotation, in Alison's own Inaugural Dissertation—*De Viribus Naturæ Medicatricibus.*) And I incline to think that it holds of any claim that may have been advanced by Dr Bennett in reference to this matter, or that may be advanced in his behalf, now that he is gone, by any of his friends.

And this brings me to the end of my task—Alison's work in relation to Harvey. Alison completed Harvey's work. He put the keystone to the arch of Harvey's work. Harvey, as I have said, had never seen the circulation in the capillaries, nor, probably, did he know anything of such vessels otherwise than inferentially. He knew nothing of the vital property of contractility inherent in the smaller arteries, and could not have thought of them as having anything to do in carrying on and maintaining the general circulation. Still less can we suppose that he ever thought of, or thought of to any purpose, what goes on so incessantly in the capillary vessels of the general system, especially in respect of the fundamental vital process of nutrition. He knew that the heart is the great moving power concerned in carrying on the circulation; and he probably thought of no other. And, albeit, after him, physiologists came to be familiar with the capillaries and the circulation through them, and with the small arteries as being possessed of a power of vital contractility, no physiologist, as far as I know, till Alison arose, demonstrated, or clearly demonstrated, these two things,—*first*, that there is a power or a force at a distance from the heart that co-operates with the heart in carrying on the general circulation, and is equally essential with the heart in the maintenance of it; and, *secondly*, that this power has its seat in the capillaries, but is independent of them; that it arises out of the changes, chemical and plastic, that go on there between the blood and the tissues, and is referable to the power—a power first clearly indicated by him—of vital affinity. This was Alison's work in relation to that of Harvey; and a great work I venture to say it was. And the principle he reached here, Dr Alison applied to the explanation of sundry phenomena connected with the circulation, but embracing also those connected with the functions of nutrition, secretion, and absorption, phenomena as well normal as abnormal, and including a great pathological phenomenon, namely, inflammation.

And I conclude by saying, that to him, in this country at least, is unquestionably due the merit of being the first to establish both the reality and the importance of a moving power in living organisms, not dreamt of by Harvey, or by any of the physiologists that went before him; a power by which the movements of the blood and other fluids in the minute vessels, and in the cellular tissues of plants and animals, are determined, independently of the heart, or of a heart or other moving solids; a power dependent on the vital processes of nutrition, secretion, excretion, aeration, and absorption going on in them, and arising out of these processes. He was the first, too, to show how large a part that principle of movement plays in the morbid as well as in the healthy processes of nutrition and secretion. He it was that, on this ground, overthrew the old theory or theories of inflammation, and elaborated that now received. The generalization now indicated was a great and an important one. It was no less than the addition to physiology of a new FOUNDATION PRINCIPLE. In his beautiful discourse on Alison delivered from this chair eleven years ago, Dr Halliday Douglas remarks that Alison was not a discoverer in the ordinary sense, but that he was nevertheless a discoverer in another sense, namely, by *induction*,—seeing principles, inferences, conclusions, in fact, which their discoverers did not themselves see. How many of the highest results in arts and arms have not had their root in discoveries of this kind? In respect of the circulation of the blood, I claim for Alison, on the footing stated by Dr Halliday Douglas, the credit of being a discoverer, and a great discoverer; and sure I am that had Alison been of the same stamp with some other physiologists, he would here have asserted his claims to be regarded as such. And as to his being on the eve of a discovery in another direction, I cannot forbear referring to a fact mentioned by Sir Robert Christison in his work on "Granular Degeneration of the Kidneys," namely this, that Alison was on the eve of anticipating Dr Bright. And so, had this happened, that disease of the kidney which bears Bright's honoured name, might now be known all the world over as *Alison's Disease.* Yet, ten chances to one if the dear Pulteney would have fared as well as Bright has done. And quite clear it is that his discovery of the power that supplements the action of the heart, in carrying on and in maintaining and sustaining the movement of the blood in all parts of the body, great although it be, and worthy of all honour, has not had the value it merits assigned to it. But it is of a piece with the little honour that has been assigned to him as a political economist. He deserved a statue here in Edinburgh—a city abounding in statues—for what he did for the poor, whom, as he said, quoting the words of the Master whom he followed, "We have always with us." Yet for this, as for other acts and deeds of his noble and disinterested life, we cannot doubt that great will be his reward in Heaven. "A sair saint" perhaps he was for Scotland, entailing on it a costly poor law. But a saint he was and a saintly man, and a great as well as a good man.

Would that Dr ACLAND, his literary executor and his intimate personal friend, who lived some time under his roof, and sat at his table, and knew him well, would yet give us an abiding record of the life and conversation of WILLIAM PULTENEY ALISON.

## ARTICLE II.—*On the Operative Treatment of Internal Piles.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

THE object of this paper is to contrast the operations at present employed for the cure of internal piles, and to express an opinion as to which of them is the best.

A large and varied experience in the treatment of this disease during my connexion with the late Mr Syme, and in my own hospital and private practice, gives me confidence in bringing forward my conclusions on this subject.

I would here remark that I am no advocate for indiscriminate operative interference in cases of internal piles, for I have learned from observation and experience as other surgeons have learned, that in many instances this affection is merely a symptom or a complication in connexion with other diseased conditions, and therefore not to be rashly treated by operation.

In the female, uterine or urinary affections, pregnancy, or the pressure of tumours; in the male, enlargement of the prostate or other diseased states of the genito-urinary organs, and in both, obstruction to the portal or general circulation from disease of the liver, digestive organs, heart, and lungs, are common causes which give rise to the disease. Unless, therefore, the cause be relieved or removed, it cannot be expected that operative measures on the piles will be satisfactory. It does happen that internal piles may so aggravate or complicate another disease as to necessitate their removal, but under such circumstances this should only be done after careful consideration, and after determining, if possible, that their cure will be beneficial to the patient. There are cases of cardiac obstruction complicated with bleeding internal piles, in which, I think, it is dangerous to interfere with the latter; and I have seen fatal results follow such interference.

The true principle of operative interference in cases of internal piles I take to be—to confine the operation to cases in which the disease is uncomplicated with other serious affections, has resisted ordinary treatment, and is causing disturbance to the general health or comfort, either by bleeding or by constant protrusion and irritation, or both.

The principle of the operation itself is to destroy or remove simply, effectually, and without hæmorrhage, the vascular growths or masses forming the piles; and in so doing to leave a sore or sores which will heal and contract safely, quickly, and thoroughly.

Although internal piles differ somewhat in their composition and appearance, it is not necessary to distinguish between the varieties as regards the question of operative interference, for practically they all require to be treated on the same principle.

The application of nitric acid may cure internal piles when there is no very distinct tumour or mass, but simply a vascular surface. This treatment is, however, unsatisfactory and ineffectual in the majority of cases, and therefore need not here be further considered. In the *Lancet* for 17th of February of this year, Mr Reeves describes a method of treating the disease by inserting a heated probe, so as to destroy the substance of the pile. In connexion with this plan, I may mention that my friend Dr Little, of Singapore, tells me that for many years he has treated internal piles on a similar principle, by making one or more small punctures in them, and then introducing a fine stick of caustic, so as to destroy their vascular structure. That either of these methods may be successful in curing circumscribed piles is surely proved by the experience of these gentlemen; but I doubt whether they are applicable, except in a certain class of cases; and when the tumours are large, the presence of one or several piles treated in this way must lead to more or less prolonged irritation, until the cure takes place.

The two operations at present in general use for the cure of internal piles are—1. The operation by ligature; 2. The operation with the clamp and cautery (Smith's). It is between these two methods I wish to judge. There are two ways of applying the ligature in treating this disease. The first is that in which the base of the growth is transfixed by a needle armed with a double thread, and each half then ligatured separately, so as thoroughly to strangulate the whole mass. The second is that suggested by the late Mr Salmon, and consists in separating with a pair of scissors the pile from its connexion with the muscular and sub-mucous tissues at the point where the mucous membrane joins the skin, and then applying a ligature round the upper and remaining attachment, which includes the vessels supplying the tumour. The strangulated pile may now be returned, or, if large, a portion of it may be cut off, and the stump and ligature returned.

This second method has the advantage that it allows the strangulated mass to separate more easily, and, as there is less tissue included in the ligature, the resulting irritation is less. Having practised this operation, I can testify to its advantage over the other plan of using the ligature.

When internal piles are treated by ligature in either way, the usual progress of the case is, that the ligature gradually ulcerates its way through the base of the tumour, and is detached along with the strangulated and dead portion of tissue at a period of from seven to ten days after the operation. If the tumour is small, the ligature may separate a day or two earlier, and, if large, the ligature may not come away before the twelfth or fourteenth day. During all this time there is more or less inflammatory action, with irritation of the rectum in the region of the ligature, and the presence of one or more putrid masses in the same canal. After the ligatures have separated, one or more sores result, according to the number of tumours treated, and these sores are not usually completely healed for several weeks longer.

Without the slightest doubt, internal piles of all kinds, and in patients of all ages and conditions, can be successfully removed by means of the ligature, and I could record numerous interesting cases treated in this way with a good result; but the operation by ligature has some disadvantages and risks, to which I will now refer. These disadvantages are:—

1. That the application of the ligatures gives rise to irritation more or less severe, and lasting usually for two, three, or more weeks.

2. That confinement to bed is, in the majority of cases, necessary until the ligatures separate.

3. That the resulting sore or sores do not usually heal quickly,

and are attended by more or less suppuration until cicatrization takes place.

4. That the operation is sometimes followed by inflammation and suppuration in connexion with the lymphatics, and also by the formation of ulcers or sinuses, which require further operative treatment.

The principal risk is pyæmia. This risk cannot be said to be great, or at all common; but in about two hundred cases of this operation, I have met with at least four instances of fatal pyæmia.

When piles are removed with the clamp and cautery, the general progress of the patient is as follows:—On the third or fourth day after the operation, the patient is able to leave his or her bed, and at the end of a week or ten days to be out of doors. For the first four or five hours after the operation, there is usually pain and irritation, which varies in intensity in different patients, and this pain generally recurs at the time of the first opening of the bowels, but there may be little or no pain from the first. From my observations, I should say that, as a rule, there is certainly much less pain and irritation after this operation than after the application of the ligature; and, from the nature and speedy healing of the resulting sore in the former operation, these symptoms are rarely present after the first week or ten days, if the clamp and cautery have been used.

In illustration, I give brief notes of the following four cases, in which the piles were removed by means of the clamp and cautery.

Case I.—Mr C., æt. 55, was operated upon in June 1872, and two internal piles, the size of filbert-nuts, removed with the clamp and cautery. The patient's bowels were moved on the third day after the operation, and on the following day he was out of bed. On the ninth day after the operation he was out of doors and quite well.

Case II.—Mr M., æt. 60, was operated upon in May 1873, and four large internal piles removed with the clamp and cautery. The patient made an excellent recovery, and was out of doors well at the end of two weeks after the operation.

Case III.—Mr R., æt. 30, was operated upon in June 1875, and three internal piles, the size of filbert-nuts, removed with the clamp and cautery. The patient was out of bed on the fifth day, and out of doors on the tenth day after the operation.

Case IV.—Mr U. was operated upon in September 1876, and three internal piles, the size of small walnuts, removed with the clamp and cautery. He was out of bed on the fourth day, and out of doors at the end of one week after the operation.

Since 1872 I have operated with the clamp and cautery on twenty-four patients, and all, with one exception to be again referred to, have progressed as in the cases just related.

The advantages of the clamp and cautery, as compared with the use of the ligature, are, in my opinion, as follows:—

1. By means of the clamp and cautery the piles are at once removed, and do not remain in the rectum as dead and putrid masses.

2. The irritation and pain are not so severe or so prolonged as in the operation by ligature.

3. The patient's confinement to bed and to the house is much shorter.

4. The resulting sores heal more quickly, and are attended with less risk of suppuration and its attendant local and general dangers.

It so happens that I can offer some strong evidence in favour of the clamp and cautery in connexion with the amount of pain and irritation following the operation, and the quickness of recovery after it—for, in three of my cases operated upon in this way, the patients had previously undergone the operation by ligature. The testimony of all these patients who had experienced both methods was most strongly in favour of the clamp and cautery.

I give a short note of these three cases.

Case I.—In 1869 I assisted the late Mr Syme to operate on Mr W., æt. 40, for internal piles. Three tumours were treated by ligature, and he was confined to bed for nearly three weeks after the operation. It was fully six weeks before he was able to go about comfortably. In April 1873, he consulted me on account of a return of the disease, and I removed with the clamp and cautery two tumours, the size of filbert-nuts. The patient was out of bed on the fourth day, and out of doors on the sixth day after the operation. He described the second operation as "paradise" compared to the first one by ligature.

Case II.—Dr —— was operated on by the late Mr Syme with my assistance in 1868. Last year, he consulted me on account of a return of the disease, and I removed with the clamp and cautery two internal piles the size of small walnuts. Shortly after the operation this patient got out of bed, contrary to orders, and strained at stool. The result was some bleeding, which, however, was checked by the introduction of a plug of lint into the rectum, and gave no further trouble. He was out of bed on the fifth day, and out of doors on the tenth day after the operation. Mr Syme's operation on this patient was by ligature, and it was then two weeks before he left his bed, and a month before he was out of doors. He expressed his strong opinion in favour of the clamp and cautery.

Case III.—In 1867 I assisted the late Mr Syme to operate upon Mr ——, æt. 53. Two large internal piles were removed by means of the ligature, and the patient was confined to bed rather more than two weeks after the operation, and was not out of doors for three weeks. Six months ago he consulted me for a return of the disease, and I removed with the clamp and cautery two internal piles, the size of filbert-nuts. He was out of bed on the sixth day after the operation, and he has recently written me as follows in regard to the two operations:—"As to the two operations, there is no comparison, the last (that with the clamp and cautery) was attended with the least sufferings, and the after-effects much better and easier than the former. It is a far superior method."

I have had similar evidence from a fourth patient, whom I assisted the late Mr Syme to operate on by ligature, and who had a second operation performed by Mr Smith himself, with the clamp and cautery.

I would here ask, Are there any risks connected with the use of the clamp and cautery? One of the principal objections which has been brought against this method is the risk of hæmorrhage after the operation. If the cautery or heated knife be properly used at an almost black heat, and ordinary precautions taken after the operation, I consider that the risk is a very slight one indeed. There has been hæmorrhage in only one of my cases—to which I have already referred—and there was good cause for its occurring. Is this operation entirely free from the risk of pyæmia? Cases have occurred, and have been reported, in which fatal pyæmia has followed the use of the clamp and cautery; and I myself have met with one case, which I will briefly relate.

A few years ago I operated on a gentleman, æt. 50, and removed, with the clamp and cautery, three large internal piles. On the fifth day after the operation the patient was out of bed, and appeared to be progressing in every way favourably. On the sixth day he had a rigor. On the seventh day he complained of pain in his side, and symptoms of pneumonia were present. On the tenth day he died and evidently from acute pyæmia.

Although, therefore, acute pyæmia may follow this operation, I am strongly of opinion that there is less risk of its resulting from the use of the clamp and cautery than from the employment of the ligature. In confirmation of this, I think I am justified in stating, that experience has shown that a wound made—especially in vascular textures—by a heated wire, knife, or other instrument, in operative surgery, is attended with less risk of pyæmia and septicæmia than one made by other means, provided antiseptics are not employed—and the rectum is a situation where they cannot be satisfactorily used.

If the clamp and cautery are used for the removal of internal piles, it is very important that the cautery or other heated instrument should be carefully applied, and at an almost black heat. I have recently employed the thermo-cautery knife in two cases to cut off the piles after they have been seized with the clamp, and I have found it most simple and efficient in its application.

As is well known, internal piles are often complicated with external piles, or with a looseness or redundancy of the skin round the anus,—and it becomes a point of considerable practical importance to consider how far such complications should be dealt with when operating upon the internal tumours. When distinct external piles exist along with internal ones,—there can be no doubt that the proper practice is to cut them off at the time of operating upon the internal tumours; but, when the condition is simply a general looseness of the skin surrounding the anus, then I think that it should not be interfered with, unless it is very marked. I have seen very troublesome results from the too free removal of such skin, which, when the internal piles are protruded, seem more redundant than it really is. The plan I myself follow is to carefully examine the external parts after the internal piles and any prolapsed mucous membrane have been thoroughly pushed up into the rectum. If then well-marked external piles or any very redundant folds of skin are present, I consider it a proper case in which to cut them off; but if the looseness or folds of skin are not aggravated, it is better not to interfere with them.

In conclusion, and as a result of my experience, supported by the facts detailed, I would offer the following opinion in regard to the ligature *versus* the clamp and cautery:—That, although internal piles may be successfully removed by the ligature, their removal by the clamp and cautery is much to be preferred.

## ARTICLE III.—*Varieties of Infecting Chancre.* By FRANCIS CADELL, F.R.C.S.

THE term infecting chancre is used to denote that form of primary lesion which is essentially syphilitic in its nature, and is invariably followed by constitutional symptoms; and is to be distinguished from the non-infecting or soft chancre, which is local in its nature and effects. In connexion with this separateness of the two kinds of sores, it may be well to allude briefly to the dual theory of venereal contagion—a theory which I believe is grounded on solid fact, and whose truth is capable of demonstrative proof in every-day experience.

This dual theory was first promulgated by Bassereau in 1852, after the most diligent and accurate clinical investigations which have ever been brought to bear on this part of the subject. He arrived at the conclusion—which Ricord after him advocated—that the hard or infecting chancre was the result of syphilitic contagion, and was always followed by syphilis; and that the soft chancre was non-syphilitic, purely local in its action, and never followed by syphilis. The only apparent exception to this rule is, on the rare occasions, when the syphilitic virus and the matter from a soft chancre are inoculated on the same spot; then the soft chancre, when the proper period of incubation has elapsed, will gradually change its character, become indurated, and be followed by syphilis.

This widely-received dual theory was referred to by Mr Jonathan Hutchinson at the late discussion on syphilis at the London Pathological Society, with more contempt than scientific accuracy. He said, "I think we may say of dualism that it is dead, and that the now simpler creed which attributes the soft chancre to contagion with inflammatory products produced by syphilis, but not as a rule containing its germs, is the one which obtains general acceptance. We have, then, in syphilis but one malady and one virus."

In this sentence Mr Hutchinson agrees with the dualists when he says that the soft chancre does not as a rule contain the germs of syphilis. He disagrees with them when he attributes to syphilitic contagion the development of the soft chancre, although he admits that it retains none of the special characteristics of its parent—a position difficult to understand. He enunciates a universally accepted fact when he says that in syphilis we have one malady and one virus.

When making this statement, "that dualism is dead," Mr Hutchinson ignored the almost universal belief in dualism which at present exists in France and America, as well as, although to a less extent, in Germany and other Continental schools; and at the same time shut his eyes to the fact that most surgical writers in this country held an exactly opposite opinion to his own. In the discussion that followed, the doctrine of dualism was defended by, amongst others, Mr Berkeley Hill, Dr Drysdale, and Mr de Meric. It is thus shown that dualism was not considered dead even in the somewhat restricted area of the London Pathological Society. How much more mistaken, then, must we believe Mr Hutchinson to be with regard to the views held on this subject by the medical world at large.

I shall now proceed to describe the varieties of infecting chancre.

1st, *The True Hunterian Chancre,* as it has been called, with indurated base and edges, and having a concave, cup-shaped appearance. It begins by a papule, which ulcerates and extends as far as the limits of the induration permit. The surface of the chancre has a glistening, smooth appearance; the edges are sloping and rounded, not abrupt; its shape is round or oval; the secretion is scanty and serous, and adheres to the surface of the sore. This is a description of the typical infecting chancre, not so frequently met with as the variety next to be described.

2d, *The Superficial Erosion.*—This type results from the formation of an indurated papule or tubercle at the seat of inoculation, whose surface becomes red, then erodes but does not ulcerate, and secretes a very scanty serous fluid. A variety of this form of initial lesion exists, characterized by the absence even of erosion, desquamation of cuticle merely taking place from the reddened surface of the lump. The varieties in form of the superficial erosion depend solely on the varying amount of indurated tissue composing its base. In one case the erosion may be on a level with the surrounding tissue; in another, slightly raised and convex; and yet again, it may have the appearance of a prominent nodule.

Another form of induration is what Ricord called "parchment induration." It is spread in a thin layer. Upon retracting a prepuce affected with this form of induration, it has a remarkable resemblance to the everted upper eyelid, both in its consistence and in the blanched appearance of the mucous membrane. Still another variety of the superficial erosion has been described by Dr Dubuc, which he calls the "multiple herpeti-form syphilitic chancre." The peculiarity of this variety seems to be, its likeness to herpes at a certain stage, and its multiplicity. It differs from herpes in having a scanty adherent secretion and in being indolent; whereas, in herpes, there is usually sharp pain and an abundant secretion of pus.

Although the primary lesion of syphilis shows itself in such different forms, the following general characteristics are common to them all:—

Their incubation is long, from ten days to six weeks; when unirritated their secretion is scanty; more or less induration is always at some time present; the lymphatic glands all over the body are indolently enlarged; and they are all followed, sooner or later, by constitutional syphilis.

## ARTICLE IV.—*On a remarkable Case of Spermatocele; occurring in Mr Lister's practice in the Royal Infirmary, Edinburgh.* By JAMES BAKER, M.B., L.R.C.S. Edin., late Resident Surgeon, Clinical Surgical Wards, Royal Infirmary, Edinburgh.

THE following case of spermatocele presents so many points of interest as to warrant a short account of it being published.

On the 5th May 1876, a man, about 60 years of age, named H. W., was admitted into the surgical wards of the Edinburgh Royal Infirmary, under the care of Professor Lister. The reason that he had gone to Edinburgh to seek surgical advice was that he had a swelling of the scrotum, which, he stated, he had first noticed about four years previously; since that time it had gradually increased in size. So far as he could remember, it had not been caused either by injury or overstraining.

On making a careful examination, there was found to be a large swelling on the left side of the scrotum, extending into the abdomen, along the inguinal canal. It was somewhat conical in shape, the apex being downwards. On percussion, the line of absolute dulness extended laterally from one inch to the right of the spine of the pubes to a point over Poupart's ligament, four inches to the left of the pubic spine, and vertically upwards for two and a quarter inches, measuring from midway between the anterior superior spine and the symphysis. The swelling was tense and firm. It did not disappear on lying down, nor increase in size on coughing, though it presented a marked impulse. On placing the fingers of one hand anywhere within the area of absolute dulness mentioned above, and pressing with the fingers of the other hand on the scrotal swelling, a sense of fluctuation was obtained with great distinctness. The testicle was to the front, and about one inch above the lower margin of the scrotum.

On May 8th, Mr Lister, using a small canula and trocar, tapped the swelling posteriorly, and evacuated seventeen ounces of a turbid, almost milky-looking fluid. Two drachms of the tincture of iodine of the Edinburgh Pharmacopœia were then injected, and thoroughly diffused throughout the whole cavity. On the following day the patient expressed himself as feeling perfectly well, and without any pain. The scrotum was considerably larger than immediately after the operation, but there was no redness of the skin. On the 10th, although he had no uneasiness when the parts were kept at rest, yet on pressure being made over the inguinal canal, pain was felt. That part of the abdomen in which, before the fluid had been drawn off, a sense of fluctuation could be obtained, was found on percussion to be resonant. The scrotum, which at this time appeared to be as prominent as ever, was noticed four days afterwards to be distinctly decreasing in size. It became less and less, until, on the 20th of May, according to the patient's account, it was smaller than he ever remembered it to have been. Fluctuation could still be elicited, but not higher than the level of the pubic bone in the inguinal canal. Three days afterwards, rather more than a fortnight after the operation, the patient was discharged from the hospital, perfectly well, never having presented any symptoms of constitutional disturbance.

It should be stated that, immediately after the fluid was evacuated, a portion of it was examined microscopically, and was found to be crowded with spermatozoa; the rest was placed on one side in a glass vessel. Twenty-four hours afterwards there was a sediment about a tenth of an inch in thickness at the bottom of the vessel, composed of spermatozoa, which, when seen under the microscope, exhibited vital movements.

It will at once be seen that the case was one of considerable interest.

Not only was the quantity of fluid unusually great, but the distance the sac extended above Poupart's ligament was especially noteworthy, being so largely out of proportion to the amount of fluid in the scrotum.[1]

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[1] In a case of Hydrocele of the Tunica Vaginalis, described by Mr Lister in this Journal, there was a large extension of the sac into the iliac fossa, but at the same time an extraordinary size of the tumour in the scrotum, reaching up even to the external ring on the other side.

Another very interesting feature of the case was the fact that spermatozoa were observed *alive* in the fluid, and that so late as *twenty-four hours after its evacuation.* I have only succeeded in finding on record two cases of spermatocele, in which spermatozoa were seen alive in the fluid drawn off. One is a case of Mr Liston's, alluded to by Mr Curling in his work "On the Diseases of the Testis;" the other is reported by M. Paul Dauvé in the *Gazette des Hôpitaux* (21st March 1867), and mentioned in the *Edinburgh Medical Journal* (Nov. 1867). In the former of these two cases, the time after its evacuation at which the fluid was examined is not stated; in the latter, moving spermatozoa were noticed seven hours after the operation.

One other point in the case, the details of which I have related, deserves mention; and that is, the admirable result of using the strong tincture of iodine of the Edinburgh Pharmacopœia. From the almost unvarying success attending its use by Mr Syme and Mr Lister, it may be looked upon, if not as a certain cure, at any rate as greatly superior to the weaker preparations of the same drug.

## ARTICLE V.—*Abstract of a Lecture on Pernicious Anæmia.* By Professor H. QUINCKE of Berne. Translated by Dr JAMIESON GUNN.

GENTLEMEN,—However often we may see patients suffering from anæmia, we can rarely content ourselves with the simple diagnosis of anæmia. We must, on the contrary, inquire into the cause of the anæmia, and we know by experience that in a great many cases the anæmia only shows itself as the result of some definite pathological condition, some marked organic disease, or febrile disorder. This is specially the case when the anæmia is associated with wasting of the body and cachexia; and if such an anæmic cachectic condition leads to death, without our being able during life to come to a satisfactory diagnosis in spite of the most minute examination, in the great majority of cases, dissection will yet reveal considerable pathological changes (deep-seated suppuration, malignant growths, etc.) which from analogy enable us to explain the anæmia and cachexia.

More extraordinary than such cases which only show the imperfection of our diagnostic expedients, and which must become always rarer by the perfection of the latter, there are others where already anatomically (during life) certain organic changes may be detected, but which do not explain sufficiently clearly the anæmia and cachexia; the peculiarly complex symptoms of Addison's and Basedow's disease belong to this category.

Finally, there are cases where even dissection has not shown, with the exception of the anæmia, any essential or constant change of any of the organs: idiopathic or essential anæmia. To this class belongs that certainly not quite clearly-defined clinical disease chlorosis, in the limited sense of the term; which, though it occurs so frequently, only proves fatal exceptionally, and through complications.

Quite recently certain cases belonging to this group have been separated by Virchow from the others, and by proof of a defective development—a hypoplasia of the vascular system—have been placed on an easily-recognised anatomical basis.

Far rarer than chlorosis, are severe cases even terminating fatally of "idiopathic" anæmia. They have certainly been observed sporadically here and there, and also have been mentioned in literature, but only within the last few years have they been described in a connected form by Biermer as "progressive pernicious anæmia."

Let us first call to your memory the description of the disease as given by Biermer.

It was found mostly among females belonging to the lower classes of middle age, preceded by disturbances of digestion, or other weakening causes. The most remarkable state was an anæmic hydræmic appearance, with general wasting, but without atrophy of the subcutaneous fat; later, slight dropsy. The usual anæmic nervous symptoms, weakness, giddiness, and palpitation, were present, besides weakness of digestion, loss of appetite, and sometimes diarrhœa; over the heart and vessels were heard loud anæmic bruits. Besides, there often existed a fever of a quite uncertain type. There was always retinal hæmorrhage, seldom petechiæ, sometimes capillary hæmorrhage into the brain and its membranes. The course was a chronic one, the issue an increasing hydræmia, with one single exception fatal.

The dissections in Biermer's cases showed, besides the general anæmia, only a partial, not a very high degree of fatty degeneration of the heart, sometimes slight fatty degeneration of the intima of the arteries, rather more fatty degeneration of the capillaries, especially of the brain, which Biermer believes to be the cause of the before-mentioned hæmorrhage. The liver, spleen, and kidneys were found normal. About the same time as Biermer, Gusserow described some similar cases of the highest degree of anæmia in pregnant women, and since then, some further casual communications have been made; lately this disease has been described connectedly by Immermann.

I will describe to you to-day ten cases of such severe anæmia, which I have observed during the last two years, and which interested me so much the more as they made me acquainted with a phase of the disease that I had not before known. Amongst the patients were four men and five women from twenty-five to fifty-nine years of age; and further, a girl eleven years old (the youngest patient, I believe, ever recorded). Certainly, not all the cases were observed so particularly as could have been wished, partly from the exceptional cause, that some only came into hospital shortly before their death; partly because the predominance of individual symptoms seemed at first to indicate a different disease; especially did this happen in my first cases, and, holding to my former experience, I was more disposed to imagine a faulty diagnosis on my part rather than the existence of a self-established anæmia. Ulcers of the stomach, cancer of the stomach, nephritis, phthisis, and typhus, were the apparent forms presented by the disease.

Allow me now to give you, as the result of the observed cases, an analysis of the anatomical and clinical facts. The first thing noticeable was the excessive paleness of the patients, which gave to the skin a transparent waxy appearance, and which was also distinctly observed in the lips and finger-nails; the face appeared slightly swollen, the features through this somewhat changed; a distinct œdema only appeared in the later stages, and was mostly confined to the feet and legs; the slight effusions into the serous cavities were seldom discovered during life. The whole nutrition was bad, most of the patients being in a high degree emaciated; while other observers in many cases were struck with the discrepancy between the very great anæmia and the bodily development generally, especially with the persistency of the subcutaneous fat. Twice there was a slight but distinct icteric colouring of the skin and conjunctivæ observed, while the urine remained free from bile.

Next to the earthy pale colour, the most striking phenomena were the great weakness and feebleness of the patients,—they were confined to bed, generally apathetic in a high degree, frequently unable to perform the movements necessary for eating and drinking. Headaches were very frequently complained of, often giddiness and fainting, especially on getting up; also the subjective feeling of palpitation of the heart was frequently complained of.

The heart's action was frequent and irregular; the pulse in the peripheral arteries small, soft, and sometimes quick. Of striking intensity were the anæmic bruits at the heart, which constantly accompanied the systole; these bruits were heard most distinctly over the pulmonary area, and occasionally assumed an extraordinary rough character. A considerable part of this bruit is most probably caused by retraction of the lungs (so common in anæmic and weak people), which also produces an apparent enlargement of the cardiac dulness. A true enlargement of the heart through dilatation of the heart, especially of the right ventricle, can sometimes be demonstrated by percussion and anatomically. Sometimes even the right ventricle forms part of the apex of the heart. The heart was generally contracted, and contained little blood. The valves were never diseased. The muscular tissue of the heart often showed a moderate degree of fatty degeneration, as pointed out by Biermer; I must, however, remark that in one case fatty degeneration did not appear even microscopically, and the muscular tissue of the heart and other organs only appeared very pale. The disease of the muscular tissue of the heart is not constant and essential, but only a secondary though frequent change in pernicious anæmia. Twice hæmorrhage into the substance of the heart and under the pericardium, and once narrowing of the aorta, were observed; over the jugular veins (and innominata) a loud bruit was heard, and a thrill felt.

The respiratory organs were free from change, with the exception of an occasional catarrh; there were no remains of phthisical changes found on dissection.

Disturbances of digestion showed themselves in almost all the cases, and seemed symptomatically rather prominent; it may be that they existed from the beginning, and partly conditioned the origin of the disease; it may be that they only appeared as the result of atonic weakness of digestion, as part of the general cachexia; only rarely was the appetite retained; in most cases an entire loss of appetite, with a feeling of oppression at the stomach after food, existed; this was often accompanied by obstinate vomiting after any kind of food. The tongue was generally clean; the epithelium even remarkably delicate; perhaps on account of this some of the patients complained of a constant pain in the tongue and a burning feeling in the mouth. In some cases constipation existed, in others an uncontrollable diarrhœa.

The anatomical changes in the digestive organs were unimportant, compared with the intensity of the symptoms; the mucous membrane of the bowels was very pale and (not always) moderately softened; a croupous inflammation of the large intestine must be considered as having been in one case the final complication. The mucous membrane of the stomach also appeared pale, and in one case of obstinate vomiting very thin; the glands of the stomach appeared less numerous than normal. Disease of the bile-ducts was not discovered even in cases where slight jaundice existed.

The liver sometimes showed slight fatty infiltration of its cells; in many cases it was of a yellowish-brown colour, contrasting somewhat with the pale colour of the other organs. I will return to this point further on.

The urine generally appeared normal; quantity, colour, and specific gravity varied much, according to the food taken; and, whether diarrhœa or vomiting existed, albumen appeared in the urine only occasionally, and, with one single exception, only to a very slight degree. No recent anatomical changes were found in the kidneys, neither fatty degeneration nor cloudiness of the epithelium.

Sometimes epistaxis existed, and in one case so repeatedly and profusely that it caused death. Once there appeared small petechiæ of the skin.

On dissection, hæmorrhagic extravasations were found in the muscular tissue of the heart, in the pericardium, on the dura-mater, and in the subarachnoid cavity.

There was constantly found in the cases examined hæmorrhages into the retina, so that this may be regarded as almost pathognomonic of pernicious anæmia. They were most abundant around the optic nerve, often along the vessels, generally profuse, not large, of an irregular long form, with their long diameter placed radially, and mostly of a light red colour. Larger hæmorrhages, which attain to a quarter of the size of the papilla, are mostly formed by the union of smaller ones. Very often in the centre of the hæmorrhagic spot a clear gray-red spot the size of a pin's point was discernible.

Sometimes, perhaps in consequence of the profuse hæmorrhages, the retina surrounding the papilla was somewhat clouded, and the boundary of the latter obscured. In another case were found the changes which were formerly considered as characteristic of retinitis nephritica, white exudative patches, and the well-known starlike speckled figures surrounding the macula lutea, besides the hæmorrhages with the gray-red centre.

Disturbances of sight in consequence of the hæmorrhages, which in some formerly published cases were very considerable, were not observed, either because the amount of blood extravasated was so small, or because the general condition of the patient was too bad to permit the sight to be tested.

The temperature of the body was in most cases normal; in others there existed a moderate amount of remittent fever of uncertain type, mostly resembling abdominal typhus, but of small absolute height, for the temperature only exceptionally exceeded 39° C. The temperature was somewhat different during profuse epistaxis and after transfusion.

Finally, as to the blood-forming organs, I have to remark that slight enlargement of the spleen seemed to be present only in the two cases that recovered; however, on account of possible mistakes, little weight must be placed on this, because, on dissection, the spleen was never found enlarged. The lymphatic glands were found only in a few cases slightly enlarged. The medullary substance of the bones was found quite normal.

*Course.*—The commencement of the disease is always gradual, the transition from health is almost unnoticeable, so that the disease is often far advanced when the patient comes to hospital, in many cases only a few days before their death. The disease generally lasted from several months to half a year, sometimes even more than a year. Generally the disease has the progressive character, as Biermer has already indicated by its name. The anæmia and the cachexia become rapidly more marked, the appetite always bad, and thus generally life is extinguished (as in a person with carcinoma of the stomach, or stricture of the œsophagus). After the bleached-looking apathetic patients, already in the last stage of existence, have become more like a corpse, and the pulse scarcely to be felt, we seldom have any final complication. However, any change of equilibrium in the feebly glimmering life may become the accidental cause of the final collapse (removal to hospital, or the relieving of a long-standing constipation).

With pregnant women in the hitherto observed cases, delivery took place from the seventh to the eighth month, and soon after (without much loss of blood) death ensued.

In the rarer cases of a favourable course the return to health is very gradual. At first the patients can take and retain something to eat, the fever abates, and quite gradually strength and colour return, yet a certain degree of anæmia always remains. A girl four years old, after several months treatment, was dismissed apparently quite well, but the disease returned after one year and a quarter, and caused her death.

The prognosis (after what has been said) is unfavourable in general, but not so absolutely fatal as has been supposed lately. Of my cases, two left the hospital as cured (in the general sense of the word); and also De Cérenville has lately reported two similar cases.

Without attaching any special prognostic value to the circumstance, I may remark that the one case which recovered, in spite of otherwise severe symptoms, only showed moderate retinal hæmorrhage; the other (which certainly later relapsed) was that of a girl eleven years old.

The prognosis is serious, according to experience hitherto (Gusserow, Gfrörer), when the anæmia occurs in pregnant women, death following a few hours after delivery.

The name chosen by Biermer denoting the course and progress of this disease—progressive pernicious anæmia—appears to me to be replaceable (from what has just been said) by the simpler one of pernicious anæmia, in so far that the disease does not always progress irresistibly, and (from analogy of the severer forms of intermittent) the term pernicious does not necessarily denote an absolutely fatal prognosis.

The diagnosis of pernicious anæmia even after careful examination is not always easy, and is only to be assumed as probable after long and careful examination. When the disease is advanced, the patients, in their general appearance, and with their complaints of headache, palpitation of the heart, indigestion, etc., mostly resemble certain cases of nephritis with commencing contraction; also the albumen found in the urine may support the idea of such a condition; but the albumen is only transitory, and the left ventricle of the heart is never hypertrophied.

Still more difficult is it in many cases to distinguish it from chronic ulcer or cancer of the stomach, if loss of appetite, vomiting, pain in the epigastrium after food, are the principal symptoms; then great weight must be placed on the former stomach symptoms, upon the simple condition of the vomit (free from blood), and upon the negative result of local examination. If, however, we reflect on the one hand how often grave anatomical lesions of the stomach are without characteristic symptoms, and if on the other we compare the symptoms of this disease, where obstinate vomiting combined with epigastric pain and slight but continuous jaundice are present, we see that in some cases mistakes cannot be avoided, or the diagnosis at least must remain uncertain.

Feverish cases, especially if accompanied by diarrhœa and general apathy, may resemble enteric fever. In one of our cases this diagnosis was made by a physician outside the hospital; only the low, not quite regular, course of the febrile temperature curve, and the absence of enlargement of the spleen, showed that we had to do with a case of anæmic fever.

Very seldom is there any reason to confound it with pulmonary phthisis. Even when complicated with bronchial catarrh, the negative result of repeated examination of the lungs compared with the symptoms will soon put one on the right track.

Disease of the heart is not readily mistaken for pernicious anæmia, notwithstanding the subjective symptoms of palpitation and the loud bruits, if we note that the blowing is always systolic, often at all four orifices, and always loudest over the pulmonary artery, with an accentuation of the pulmonary second, caused by an abnormal contraction of the lungs. The other phenomena also neither accord with disease of the valves nor with acute endocarditis.

Finally, the ophthalmoscopic appearance of the eyes is of the greatest diagnostic importance, and is seldom wanting in advanced cases; and in its usual form, on account of the quantity of blood effused and the absence of true retinitis, is not likely to mislead, especially as it is mostly observed in young persons. The gray-red centre of this extravasation appears to be in some degree characteristic. I have mentioned already that in otherwise cachectic individuals (carcinoma, etc.) retinal hæmorrhages have only been found exceptionally. On the other hand, it is important to know that the appearance of Bright's retinitis may occur with pernicious anæmia, and therefore must not be taken as pathognomonic of nephritis.

If we now inquire into the cause and origin of this particular form of anæmia, the accumulation of the cases hitherto observed in Switzerland and the adjoining parts of Germany is most striking, and forces upon one the supposition, that here, perhaps in the mode of life or food, predisposing conditions are more frequent than elsewhere. What these conditions are cannot at present be determined. Certainly, as the literature shows, similar cases have also been observed sporadically elsewhere.

Relatively, the disease appears to occur more frequently during the gravid state, and perhaps represents only the excessive pathological development of that anæmic hydræmia, described by many authors as normal in this condition.

In most of my patients poverty could be proved with certainty; poor, insufficient food, often consisting of only coffee and potatoes, kept them alive; and hard work, frequent pregnancies and lactations, severely taxed the powers of the organism. Frequently other unfavourable circumstances co-operated, such as the habitual use of alcohol, repeated epistaxis, recovery from typhus, and tedious catarrhs of the stomach and intestines.

Even if we have to admit that apparently the same injurious circumstances continue to operate upon a great number of poor people, without their falling victims to pernicious anæmia, nevertheless, this cannot be accepted as any reason for refusing to them an etiological connexion with this disease, for, on the one hand, we do not know to what amount of unfavourable circumstances a single individual has been exposed in the course of time; and, on the other, the individual powers of resistance is very variable, as experience teaches us in relation to epidemic and endemic diseases, and further, in acquired pulmonary phthisis, to which only a fraction of those individuals fall victims, who apparently have been exposed to similar unfavourable circumstances as to work, air, and food. Also the obstinacy and the irresistible course of the anæmia appears intelligible, for if it has once attained a certain degree, it leads to dyspepsia, vomiting, diarrhœa, perhaps even to disturbances of nutrition in the blood-forming organs, and thus a *circulus vitiosus* is formed, from which the organism can only free itself with great difficulty, if at all.

Some of the cases one can therefore, as I believe, explain sufficiently, according to our former experience, and we do not need to accept with Immerman the absence of any sufficient etiological cause as characteristic of pernicious anæmia. Protracted inanition is perhaps the most suitable expression for this mode of origin.

But we can by no means trace all cases of pernicious anæmia back to this origin. In some of the cases described in the literature of the subject, the disease originated under favourable surrounding circumstances, also in some of my cases no specially unfavourable circumstances could be proved.

We must therefore look for other causes. But before I commence to speak of the various possible modes of origin, let me first tell you something about the state of the blood and some of the other organs, and this only supplementarily in this place, in order that we may enter in this connexion at once upon the different modes of origin of the disease.

As already stated, the blood was always diminished in quantity, the heart and vessels being only partly filled, at the same time the blood was of a light colour, thin, and coagulated badly.

A drop of blood taken from the finger-point during life appeared very pale, the number of blood-corpuscles in a microscopic preparation were very scanty; it was not possible for me to ascertain the absolute number, as Malassez's apparatus had not then come into my possession. In several cases the unequal size of the red blood-corpuscles struck me, for, besides those of the general form, a small, but by no means an insignificant, number with smaller diameter were found, similar to those forms described by Vanlair and Masius as microcytes. In three of the cases mentioned many of the red corpuscles, especially of the smaller kind, presented strange differences of form; they were oval, long, crooked, sometimes they were drawn out and provided with a pointed appendage; quite small yellow-coloured particles were also found (changes in these singular forms were observed several times under the microscope, but they could scarcely be called vital processes).

The larger of these first-named particles one might take for defectively formed hypoplastic blood-corpuscles; in regard to the smaller irregular formed ones, it seems to me most likely they are formed by the decay and disintegration of the red blood-corpuscles.

In some cases between the blood-corpuscles I found very abundantly, sometimes in groups and heaps, sometimes singly, those now dull, now glistening, fine granular masses, so often seen in the blood of cachectic individuals, reminding one in their appearance of the white blood-corpuscles, and which most likely owe their origin to their disintegration. In one case they were so numerous that the blood-serum appeared muddy.

In another case, with absolute diminution of the number of red blood-corpuscles, these colourless particles appeared remarkably numerous, so that a relative leucocythæmia existed, which was caused by the disintegration of the red corpuscles, perhaps also by the simultaneous increased formation of the colourless elements.

In no case was the transition between the red and white corpuscles seen, as is observed during convalescence from acute loss of blood.

Therefore, from some of our cases there are results which point to a profound decay of the cell elements of the blood, sometimes the red, sometimes the white, while reparation by new-formed elements appears wanting.

What share the spleen, the lymphatic glands, and the medullary substance of the bones have in this anomaly, whether it is a functional sluggishness of these organs or a defective formation of the cell elements, perhaps also an abnormal profuse destruction of the same (for example, in the spleen), does not appear from our previous examinations; on the other hand, there appears to me to be another condition, not without interest, which perhaps might account for the increased destruction of the red elements in another organ, viz., a strikingly increased quantity of iron in the liver, which, in three cases, was ascertained quantitatively and micro-chemically, while in one case it was not present. The quantity of metallic iron was in the two first cases 2·1 and 0·6 per cent. respectively of the dry substance of the liver; the iron was contained in the cells of the liver itself. In all three cases the small and singularly shaped red elements were found in the blood.

Certainly the interpretation of this condition is rendered more difficult from the fact that two of the cases also showed an abnormally high quantity of iron in the kidneys and in the pancreas. Further, the possibility of the storing up of iron (given formerly medicinally) in the liver cannot be excluded.

If we review once more the facts just stated, I believe you will see that the state of the blood and the other organs are not always the same in pernicious anæmia, and that most probably the disease arises in very different ways, and that we have not to do with a uniform process of disease; but that pernicious anæmia, like anæmia in general, may be the product of very different morbid processes, being, as it were, its highest development, represents the last stage of anæmia. We may, therefore, in a certain sense, compare it with uræmia, jaundice, and septicæmia, which likewise present typical forms of disease that may, nevertheless, have originated from very different causes.

The consequences of the extreme change and diminution of the blood mass are, on the whole, the same, but differently expressed in each case. As such we take the general weakness, the œdema, the fatty degeneration of the heart, the disturbance of the appetite, the diarrhœa. The albumen may just as likely be the result of an altered secretion of the kidneys, caused by anæmia, as of an altered composition of the blood serum. The fever may owe its origin directly to the altered activity of the nervous heat-regulators (in the widest sense of the term), just as likely as to the abnormal tissue change, and may thus be analogous to the fever observed by Chossat in starved animals. The hæmorrhages may depend upon the changed nutrition of the capillary walls, or, as the state of the retina seems to indicate, upon the local accumulation of the white globules.

In individual cases the features of this form of disease may be modified by its mode of origin, impressed, as it were, by a residue of the original process; and, in reference to this, we must bear in mind that defective new formation (anhæmatose), and increased destruction of the blood elements (hæmophthisis), form the two principal types, which certainly may be combined with each other, and with which the want of certain morphotic and chemical elements may be more or less predominant, and thus give to the disease in different cases a somewhat diverse colouring.

Examples of the first type would be found in cases of chronic inanition, atrophy of the mucous membrane of the stomach, or in such trophic disturbances of the blood-forming organs as was supposed by King and Schüle to exist in the cases described by them. Examples of the second kind of origin are perhaps found in the cases with increased quantity of iron in the liver.

The result of such a comprehensive idea of pernicious anæmia is, that the boundary between it and the intensified forms of the so-called simple anæmia is not very sharply defined; and indeed many such doubtful cases have been seen by myself, and are also recorded in the literature of the subject.

It will be the duty of future inquirers to define distinctly the differential diagnosis between pernicious anæmia and other allied diseased conditions, as well as to point out the connexions which subsist between them; this, too, will be the only way to promote a right understanding of the disease.

The treatment of pernicious, as with other forms of anæmia, resolves itself first of all (where possible) into the removal of the cause. The patients must at once be removed from their unhealthy home influences, and nursed in an hospital. In some cases (De Cérenville), the most careful hospital nursing availed nothing; but recovery took place when the patients were sent to the hills

The greatest attention must be paid to the feeding of the patients, especially as loss of appetite, vomiting, or diarrhœa are so often present. These disturbances are to be combated symptomatically by the usual remedies; yet I must not withhold the fact that these often entirely fail.

Next to carefully-chosen diet, hydrochloric acid, rhubarb, and gentian render the best service; the preparations of iron I employ after regulating the functions of the stomach. During the feverish state I employ quinine, and to assist convalescence lukewarm baths were used.

In pregnant women, the experiment of producing premature labour from causal indications has been recommended by Gusserow.

An obvious remedy, apparently very rational, recommended by various parties, is, lastly, transfusion of blood, which I have employed on several occasions.

Leaving out of the question the transfusion of lamb's blood, which recent experimentation seems to give good ground for abandoning, the transfusion of human blood was not followed by successful results, principally, perhaps, because it was employed during a period of the disease when the altered composition of the blood had already caused too great an injury to the functions of the organs, especially of the digestive system and the heart.

In one case, the numerous fresh hæmorrhages which were found on dissection must certainly be regarded as the result of the transfusion, although the injection into the arteries was very slowly made, and any essential rise in the blood-pressure during injection could scarcely be accepted from analogy of physiological experiments. With the already existing tendency to hæmorrhages, the change in the composition of the blood seems to have been sufficient to cause a profuse extravasation of blood. At all events, this observation cautions us rather at first to transfuse smaller quantities, perhaps only 100 grammes, at a time.

In cases where the formation of blood is prevented by primary disease of the blood-forming organs, we can obtain only a very temporary benefit at the utmost from transfusion, even when employed at the right time; but where, from chronic inanition, reparable disturbances of digestion, or where, by the loss of a large quantity of vital fluids, an anæmia even of a high degree is caused, we may always hope to break through by transfusion the existing *circulus vitiosus*, and to supply to the body for some time sufficient working material to enable digestion and blood formation to get into a normal state, and so to maintain the body. So long as we are unable to distinguish this latter curable from the former incurable form, the trial of transfusion will be justified in any case, and the difficulty will be especially, on the one hand, not to transfuse uselessly, and, on the other, not to do it too late. This question can only be decided by a judicial appreciation of all the circumstances of the case.

## LUNACY ADMINISTRATION IN SCOTLAND.

Our readers may have observed during the last week of April that the administration of asylums, and several of the other details connected with the management of lunatics in Scotland, received very special attention in three of the London weekly medical papers. Two of these journals make the evidence given by one of the Scotch Commissioners in Lunacy before the Select Committee of the House of Commons on the Lunacy Laws the ostensible occasion of the articles they have devoted to the subject; the third, without any apparent occasion, registers a serious indictment both against the system of the Scotch administration, and against the persons by whose labours it ought to be carried out. The *Medical Times and Gazette* and the *British Medical Journal* comment on certain opinions said to have been expressed by Sir James Coxe in his evidence before the Committee. The *Times and Gazette*, adopting a course fortunately unusual in respectable journalism, tries, by an ingeniously constructed biographical sketch of his professional life, to discredit his views upon all that relates to lunacy. It is sufficient to condemn the writer of the article that he should have introduced such an element into the discussion; and we feel that it would be impossible for Sir James, or for any one else possessed of ordinary self-respect, to reply in detail to such argument. It must be some consolation to him, however, that when the cloud of verbiage is cleared away, what is relevant of the statement amounts to only this,—that, previous to his being selected for his present important post, he had never been an asylum officer. We do not desire to make light of the valuable experience that is acquired by residence within the walls of an asylum; but it cannot be held to be an indispensable qualification for the office of Commissioner; and indeed it might be reasonably maintained that the members of the Superintending Board should not be exclusively selected from among asylum physicians. It seems only necessary, in support of such a view, to recollect that neither Pinel nor Conolly had served such an apprenticeship when they were called on to take those most important positions which they held with so much honour while carrying out the great reforms in the treatment of insanity with which their names are associated.

The article in the *British Medical Jovrnal*, though not so offensively personal, has a strong personal leaning. It makes an attack, however, upon all the officials of the Scotch Board. It makes strong assertions. According to it, the Board has asserted "that asylums are egregious blunders," and that it "has effectually succeeded in weakening the public confidence in lunatic asylums in Scotland." We look in vain for any quotations or record of facts in support of these assertions. They are serious charges, and ought to be substantiated when made; and, in regard to the first, not only can we find no justification for it in anything which has issued from the Board, but we find a great many statements implying the direct opposite. The article abounds, however, in reckless statement. Sir James is reported in the article to have said in his evidence that "asylums are only prisons after all." We have no means of knowing whether this is an accurate report or not, as the minutes of evidence have not been published yet. It is quite open to discussion, however, how far asylums are or are not prisons. And this, at least, is evident,—they are places into which some, at least, of the inmates are put because it is necessary for public order and safety that they should be kept there, and kept both against their will and independent of whether such confinement is the best medical treatment of this condition. But the writer is so eager to raise a storm of indignation against Sir James for the alleged statement, that he indulges in the following absurd comparisons: "An asylum," he admits, "is a place of confinement and restriction of liberty, and to that degree it is a prison; but," he adds, "to the same degree a public school is a prison, for the boys may not break bounds, nor enter certain parts of the building that are not allotted to their use; and a hospital is a prison, because the patients are restricted to their own wards; while to an even greater extent barracks and workhouses are prisons. Nay, if any interference with absolute liberty constitute the place in which it occurs a prison, then are our public offices of that description, because the clerks in them are cooped up in certain rooms from ten till four each day, and are compelled to drive the quill, sort papers, and cipher assiduously, with only an occasional glance at the *Times* or *Scotsman*. Then are our factories and our workshops prisons, because operatives are immured in them daily for long and weary hours; then are our cities prisons, because every man may not enter his neighbour's house; then is the world a prison, because its inhabitants cannot visit Mars and Saturn at pleasure."

We have now to notice the article in the *Medical Press and*

*Circular;* and here there is less difficulty in dealing with the statements that it contains; because, fortunately for those who are attacked, the writer ventures upon some assertions regarding matters of detail that can be readily inquired into. To enable our readers duly to appreciate the nature of the accusation, we must be permitted to place a rather lengthy quotation before them. The aim of the writer is, he says, "to point out a practical evil." "That practical evil is the neglect of what are called the single patients in Scotland (that is to say, lunatics residing with their friends, or boarded out) by the Commissioners and Deputy-Commissioners, and the withdrawal from them of that supervision which has been regarded as essential to secure their freedom from cruelty and ill-usage of every kind. The Report of the Board of Lunacy reveals that a considerable proportion of the lunatics in private dwellings in Scotland are overlooked by the Board only in the passive sense of the word. The total number of the insane in private dwellings on the 1st of January 1875 was 1472, and the total number of private patients visited by the Deputy-Commissioners was 1227; so that 245 patients in private dwellings were not visited at all during 1875. It will certainly surprise the public to learn that 245 lunatics, mostly paupers, were left in Scotland for at least twelve months, probably for nearly two years, without any of that official inspection that has been prescribed for them by Act of Parliament, and the value and importance of which the General Board of Lunacy in Scotland has so often and so strongly insisted on. Surely such a state of matters cannot be allowed to continue. It is out of the question that so large a number of helpless persons should be left unreservedly to the tender mercies of those with whom they live, and who in many instances receive very inadequate pittances for their maintenance. . . . . Their lot is at best servile and squalid. We fear that it is now sometimes intolerably wretched and burdensome. We are far from attaching an extravagant value to one annual visit by a Deputy-Commissioner, and should much rather know that numerous visits were paid at short and irregular intervals; but still even one yearly visit is some check upon those who have the care of the insane poor, and some guarantee that they shall not practise atrocities upon their charges with impunity."

There are two distinct charges made here. One refers to the condition of the pauper patients in private dwellings in Scotland. It is said that "their lot is at best servile and squalid," "sometimes intolerably wretched and burdensome," and that "atrocities" are practised upon them with impunity. The other refers to the manner in which this supervision is conducted; and in this the officers of the Board of Lunacy are said to have sadly neglected their duty. As regards the condition of the patients, it is incumbent on the writer to show some evidence that his description is just. In the meantime our readers must accept the fact that such a description belies all that has been written of late years by any writer who has thought it safe or convenient to authenticate his statements by his signature. If it should ever be attempted to justify such a description, it would be necessary that the facts given in its support should be complete. No collection of a few cases such as those annually given by the officers of the Board of Lunacy themselves as exceptional instances would suffice. As well might one describe the condition of all asylum patients as unsatisfactory, because lamentable accidents still occur within their walls in spite of an administration that is justly described as careful, enlightened, and humane.

But the other count of the indictment can be met at once. 1472 patients, it is said, were in private dwellings under the supervision of the Board in the year 1875; and of these there were 275 unvisited during the year. We may take this statement as if it were strictly accurate. It is unnecessary to observe that those two happy Deputy-Commissioners, who are described in one of the articles as having been continued in office merely to be agreeable colleagues to the Commissioners, must have had no little wandering to and fro to reach the 1227 who are scattered from Unst to the Mull of Galloway, and from the Hebrides to Buchan Ness. But the complete and sufficient reply to the accusation that lunatics in private dwellings are neglected in Scotland is, that the whole 1472, besides the inspection by the Deputy-Commissioner, are under supervision not only by the inspectors of poor of their several parishes, but they also receive four visits each year from the parochial medical officers, who record in a register their opinion of the way in which each patient is clothed, fed, and cared for. And if nothing else could be urged, we should feel warranted in believing that twenty per cent. of the patients in private dwellings may with safety be left for more than twelve months under such supervision. We believe, indeed, that in several districts the visits of the Deputy-Commissioners might safely be paid at longer intervals than they are at present. For though inspection of some sort is an undoubted advantage both to the patients and also to the local officers, we believe that in many instances the patients would be well cared for without any such supervision. The fact of the existence of the local supervision is not alluded to by the writer of the article in the *Press and Circular;* and one might have hoped that he was ignorant of it, had not Dr Major, of the West Riding Asylum at Wakefield, writing evidently under the belief that the writer, if acquainted with this fact, could not have failed to see how it disposed of all his allegations, pointed out in next number what he supposed to be this oversight. To Dr Major's letter the following editorial note is made, showing that the omission of all reference to the local inspection was of set purpose.—"We are well aware that pauper lunatics boarded out in Scotland are visited by a local medical practitioner and by the inspector of poor in the manner referred to by Dr Major; but our contention is, that such local inspection is practically of little value in preventing abuses without frequent supervision by a central authority."

Even when a writer has been shown to have such a talent for omitting those facts which might let the groundlessness of his accusation be seen, our readers may perhaps be scarcely prepared for the statement of another fact, of which this writer cannot be ignorant. There are in England, where it is implied by the writer that the administration is much better than in Scotland, upwards of 6000 pauper lunatics boarded with relatives and others; and these persons are under no supervision except by the local officials. The local medical officers there, as in Scotland, are required to pay four annual visits to each patient, and to report upon his or her condition; but no Commissioner or Deputy-Commissioner makes any regular inspection on the part of the Government. If, then, the condition of the Scottish lunatics is necessarily so terrible because some 275 of those not in asylums have been left for more than a year without government inspection, what, we may ask, must be the state of the thousands in England who never receive such inspection at all? We have no reliable data on which to found an answer to this question, but we have sufficient confidence in the poor-law medical officers of England to prevent us from concluding that these thousands are generally in a state "at best servile and squalid," or the victims of "atrocities." If, however, the attack now made on the Scotch administration were justifiable, we should be inevitably forced to accept this sad conclusion. An impartial comparison of the Scotch and English systems would probably be attended with advantage to the lunatics and the public of both countries; and we should rejoice to see such an inquiry honestly undertaken by any of our London contemporaries. But we can foresee nothing but evil results from such writing as we find in the articles now under consideration. We have difficulty in understanding the motive which has inspired such writing; but there is a similarity in the tone of the whole that suggests the belief that all the articles are by the same pen or inspired from the same source. It is always injudicious to impute motives, even when they can be ascertained with certainty. But we cannot in this case see any reason to justify us in supposing that a desire either for the public good or for the welfare of the insane contributed in any way to the organization of this attack.

# Part Second.

## REVIEWS.

*Dangers to Health in our own Houses: A Lecture.* By T. PRIDGIN TEALE, M.A., Surgeon to the Leeds Infirmary. 8vo., pp. 32. London: Churchill.

THIS pamphlet is the substance of an address delivered before the Leeds Philosophical and Literary Society, where it was received with so much favour that the author was requested to re-deliver it to the working-men of Leeds. A more telling statement we have rarely read. Without pretending to be a sanitarian in the strict acceptation of the word, and to be technically familiar with the details of sanitary engineering, Mr Teale in a small space has managed to lay bare, in a most telling manner, the dangers of the ordinary system of house-drainage. By means of examples drawn from property belonging to himself and also from the houses of patients where disease had given the first warning of serious sanitary defects, he convincingly shows it to be the duty of every householder, be he tenant or proprietor, to have his house drains examined by competent tradesmen; and he points out that the expense thus incurred is money well spent, and bearing good interest in freedom from illness and death. The accompanying illustrations are very telling, and enforce our author's statements. The pamphlet is a marvel of cheapness, being published at a shilling.

*Recherches sur les Centres Nerveux: Pathologie et Physiologie Pathologique.* Par le Dr V. MAGNAN, Médecin de l'Asile Sainte-Anne. Avec 3 planches, et 2 figures. Pp. 222. Paris: 1876.

THIS volume contains a collection of essays by an accomplished physician already well known for his original researches as a clinical psychologist. The *first part* relates to *general paralysis*, and contains four essays, viz.—1. On colloid degeneration of the brain in general paralysis; 2. On the lesions of the ventricular walls and subjacent parts in general paralysis; 3. On the anatomico-pathological study of general paralysis; 4. On the relation between lesions of the brain and certain lesions of the spinal cord and nerves in general paralysis. The *second part* contains four essays, respectively entitled — 1. Physiologico-pathological researches relating to epilepsy, alcohol, and the essence of wormwood; 2. Disturbances of the intellectual faculties and of the senses in acute and chronic alcoholism; 3. Hemianesthesia, general sensibility and the senses in chronic alcoholism; 4. The strait-waistcoat. The *third part*, which relates to the statistics of insanity in the department of the Seine, contains two essays entitled—1. Statistics of alcoholic patients received at the admission-office [*bureau d'admission*] of St Anne's Asylum during March, April, May, and June 1870, and corresponding months of 1871; 2. Statistics of patients received at the admission-office in 1870 and 1871.

The essay on *colloid degeneration of the brain* gives a very complete account of this rare mode of the ultimate evolution of the usual fundamental lesion of general paralysis, viz., diffuse generalized interstitial encephalitis. Colloid substance has characters peculiar to itself, which distinguish it from other morbid cerebral formations to which it has some general similarity. It is not of a greasy nature, for it does not affect the colour of carmine, and is not dissolved by ether or chloroform. It is not inorganic, for it is not acted upon by hydrochloric acid. It is distinguished from amylaceous bodies by not being affected by iodine, by its solubility in concentrated acetic acid, and by its being scarcely influenced by cold solutions of soda and potash. Most of the preparations showing the colloid degeneration of the brain were examined by Professor Charcot, who has furnished delineations of some of them, which increase the value of the author's descriptions. We commend this essay to the study of those interested in cerebral pathology. Dr Magnan has added considerably to the information given by Rokitansky and others on colloid degeneration of the brain.

The essay on the *lesions of the ventricular walls and subjacent parts*—the conjoint production of Dr Magnan and of Dr Mierzejewsky—was originally published in 1873. Its general teaching is thus summed up in the concluding paragraph:—"The chronic irritation which manifests itself at the periphery of the encephalon and in the ventricular cavities, with its extensive diffusion in all directions, demonstrates that the lesion of general paralysis is really a diffuse general interstitial encephalitis. The explanation of its being more marked in some situations is its there finding more favourable elements for chronic inflammation, viz., cellular tissue and a greater abundance of vessels."

The essay on the anatomico-pathological study of general paralysis originally appeared in 1868. The seven general conclusions arrived at by the author are to the following effect:—

"1. In general paralysis the lesions visible to the naked eye possess no value except as regarded in the aggregate.

"2. They are liable to modification by purely physical conditions irrespective of the disease. It is not unusual for one or more of them to be absent: in some exceptional cases, they may all be wanting.

"3. The intimate lesions of the brain—general nuclear proliferation of the interstitial tissue—have seemed to be constant, irrespective of external action: consequently, they are the lesions which ought to be chiefly looked for.

"4. In medullary lesions—propagation of the lesion from the spinal cord to the brain, or from the brain to the spinal cord—the morbid process may be followed up to its complete development, thus affording a much better explanation of its different phases.

"5. The primary seat of this lesion is in the interstitial tissue: when parenchymatous lesions exist, they appear to me to be consecutive.

"6. The parenchymatous lesions show themselves very early; and the existence of granular bodies, easily recognised when the spinal cord is in a fresh state, enable them to be better distinguished.

"7. The gaps, or areolæ of disintegration, observed in the spinal cord, may, at a future date, when their nature has been determined with more precision, explain the partial paralysis of motion and sensation generally attributed to obscure disturbance of the circulation."—P. 56.

The essay on the *relations between lesions of the brain and certain lesions of the spinal cord and nerves* is the reprint of a lecture delivered at S. Anne's Asylum, and published in March 1871 in the *Gazette des Hôpitaux.* The author maintains that the intimacy of the relations which exist in general paralysis between lesions of the nerves, spinal cord, and brain, shows that the leading fact in the development of the disease is the general tendency of the entire nervous system to take on a peculiar kind of irritation which presides over the multiple local determinations.

The experiments on animals and clinical *researches* of Dr Magnan *in relation to epilepsy, alcohol, and wormwood* are most important and highly original. They were made known favourably and widely in Great Britain and other countries by the prosecution instituted in 1874 by the "Royal Society for the Protection of Animals" against Dr Magnan and others. The summonses were taken against "Dr V. Magnan of Paris;" and against "Mr Haynes S. Robinson, Mr John Ballard Pitt, Mr Richard Wentworth, and Mr Horace Turner, medical practitioners residing at Norwich," at the instance of "Mr John Colam, Secretary of the Royal Society for the Prevention of Cruelty to Animals, for that they on the 14th day of August last, in the smoking-room at the Freemason's Hall, in the parish of St Peter, Mancroft, in the said city and county of Norwich, did unlawfully ill-treat, ill-use, and torture certain animals—to wit, two dogs—contrary to the statute." The proceedings were taken under the second section, chapter 92, of the 12th and 13th Victoria, which enacts that ' if any person should cruelly ill-treat, ill-use, or torture, or cause or procure to be tortured, any animal, he should forfeit an amount not exceeding £5." The Act declares that persons who aid, counsel, and abet, are principals in the commission of the offence; which is the reason of the above-named gentlemen having been summoned. Of course Dr Magnan did not travel back from Paris to Norwich in obedience to the citation; but his "aiders and abettors" were tried at great length before a bench of the local magistracy. Evidence was read to the effect that Sir James Paget, who was President of the British Medical Association at the Norwich meeting, had sanctioned Dr Magnan's experiments, and declared them to be "important to science and calculated to increase our knowledge of the nature of disease and our means of relieving human suffering." The late Sir William Fergusson of London and Mr Thomas Joliffe Tu nell of Dublin in person gave evidence, more strong than sensible, on the other side. Sir William Fergusson, according to the newspaper report of the proceedings published at the time, stated that "the experiment would not benefit science, as it was not likely that one person would inject absinthe or alcohol into the vein of another." "It was," he added, "an act of cruelty to fasten a dog down upon a board, make an incision, insert a tube, and inject absinthe." The magistrates dismissed the case; but the support given to the prosecution at so much personal inconvenience by Sir William Fergusson and Mr Tufnell originated an agitation, known as "the anti-vivisection movement," which led to the appointment of a Royal Commission to inquire into the subject of scientific experiments on animals, the outcome of which Commission was a statute sanctioning them under certain conditions. The manner in which the weak, well-meaning people, who constitute the majority of the pseudo-angelic section of modern society, delighted (as usual) to be duped by the nonsensical mendacity of a few unscrupulous stipendiaries and childish amateurs, constituted a remarkable feature in the agitation which took its origin in the two experiments on dogs which Dr Magnan of Paris performed at Norwich in 1874 in the interests of science and humanity. Having said thus much regarding a curious episode in the history of medico-scientific progress, we complete our account of it by giving the resolution passed by the Local Executive Committee of the meeting of the British Medical Association held in Norwich. We refer for fuller details of the trial to the *British Medical Journal* for 12th and 26th December 1874:—

"The Local Executive Committee acting for the annual meeting of the British Medical Association at Norwich in August last maintain that the invitation to M. Magnan was justifiable. They believe that the repetition in England of an experiment calculated to throw light not only on the effects of a dangerous dietetic agent, but also on the obscure pathology of convulsive nervous diseases, was advisable, remembering that by similar experiments, assisted by money grants from the Association, an important advance in therapeutical knowledge has been recently established. Whether, however, the Committee acted wisely or not in granting M. Magnan the opportunity he requested, they cannot refrain from expressing their surprise and regret that Sir William Fergusson, the late President of the Association, and Mr Tufnell, should have lent the weight of their influence in aiding a criminal prosecution against five gentlemen who were not only professional brethren, but four of whom were simply acting as secretaries on behalf of the British Medical Association."

The essay in the collection now before us is an abstract of part of a larger work which the author published in 1874, entitled—"*De l'Alcoolisme des Diverses Formes, du Délire Alcoolique, et de leur Traitement.*" We commend this work, which has been translated into English by Dr Greenfield, to the particular notice of our readers, both as presenting a model of clinical research and as containing a large amount of practical information. The liqueur called "absinthe" is the most injurious of all the intoxicating drams of the cafés and tippling-shops of Paris. It is a complex compound, the active ingredients of which are alcohol and wormwood (*absinthe*). The effects are also complex. Dr Magnan determines the respective action of each. He describes the drunkenness produced by alcohol, with its stages of excitement, stupidity, paralysis, anæsthesia, and coma. He then shows that the phenomena produced by wormwood are of a different kind, viz., muscular twitchings, giddiness, epileptic fits, hallucinations, and delirium.

From the author's prefatory summary we translate a few sentences. He says:—

"In a dog under the observation of the author, the prolonged use of alcohol successively originated, changed ways and disposision, illusions, hallucinations, delirium, and trembling, together with disturbance of the digestive functions and the other symptoms which occur in alcoholized human subjects. On the other hand, the autopsies made under rigorous experimental conditions threw great light upon the pathogenesis of alcoholism. According to the different degrees of the affection we saw steatosis of the liver, kidneys, and heart, also chronic inflammation of the meninges, spinal cord, pericardium, and coats of the stomach.

"The essence of wormwood administered in small doses occasions giddiness and convulsive twitchings in the head and anterior parts of the body; in large doses it causes epileptic seizures and delirium. The latter, it is important to remark, results from the first impressions produced on the economy by the absinthe; alcohol requires, on the other hand, several days to prepare for itself a soil fitted for the production of hallucinations and delirium.

"When alcohol and the essence of wormwood are simultaneously administered, the first result is alcoholic drunkenness: then follows the absinthic attack.

"The facility and rapidity with which the injection into a vein of essence of wormwood causes an epileptic attack afforded the opportunity of making a series of experiments with a view to determine during the fit the degree of action in the brain, medulla oblongata, and spinal cord, by examining the bottom of the eye and the surface of the brain to ascertain the state of the circulation in the retina and in the brain during the different stages of the attack.

"The results of these experiments are not in harmony with the received physiological theories of epilepsy, and specially tend to negative the opinion that the medulla oblongata has in itself the power of giving rise to the epileptic attack; they show that the cause is more general, and that it acts, in the more complete fits, upon the entire cerebro-spinal axis."

After establishing the distinctive symptoms produced by alcohol and wormwood, Dr Magnan gives an account of his physiological and pathological researches into the progressive action of alcohol and wormwood, separately and combined, upon the economy in respect of the physical and intellectual disturbances which they produce, and the anatomical lesions which they originate. Delirium tremens is most instructively considered.

*The strait-waistcoat* is described and justly condemned by Dr Magnan. He states that it is now almost the only engine by which mechanical restraint is employed in medicine; and "it is," he says, "in general use, not only in asylums, but likewise in ordinary hospitals." This remark of the author shows that the disuse of mechanical restraint is, as yet, very far from having become the rule in France, as it has long been in Great Britain. Though Dr Magnan disapproves of the strait-waistcoat [*camisole de force*], he does not reject the aid of mechanical contrivances to prevent patients from injuring themselves and others. For the strait-waistcoat, he substitutes a dress, the basis of which is a simple arrangement of swaddlings, to which he applies the term *maillot*, the same word which designates the swaddling clothes of an infant. An account of the *maillot* in detail would occupy a good deal of space; and, unless it were illustrated by the wood-engravings accompanying the author's description, it would not convey a clear idea of the contrivance. We therefore refer those who are interested in the subject to the volume now under review, or to an examination of the *maillot* itself, which can be seen in Dr Magnan's wards at Sainte-Anne's asylum. The great advantages of the *maillot* are its non-interference with respiration, its restraining the patient from doing mischievous violence with legs and arms, its affording—at a minimum of mechanical restraint—security against homicide, suicide, escape from custody, and other dangerous outbreaks. It is a mode of applying restraint which does not involve the risk of the bruises, fractured bones, and internal injuries which have too often resulted from attendants carrying out, in a brutal or ignorant manner, what is called "the non-restraint system." Is the *maillot*, or the grip of attendants—often excited and on the defensive—the least cruel method of employing physical constraint to patients? We commend to the notice of all practitioners Dr Magnan's discussion of this question, which does not belong exclusively to lunacy practice. General physicians have frequently to use physical restraint in cases of puerperal insanity, delirium tremens, and fevers.

*The medical history of the two sieges of Paris*, with an account of their immediate and more remote effects upon the community, will, we trust, be truthfully written by an impartial and competent physician. The following extract will constitute a valuable contribution of materials to such a work. It exhibits some of the statistics of insanity in Paris in 1870–1871, when the city suffered from siege, civil war, cold and darkness, disease and famine, bitterly aggravated by a pervading feeling of national helplessness and humiliation. The effects, as shown by statistics, are not such as might have been expected from the causes.

"The medical care of persons of unsound mind in the department of the Seine," says Dr Magnan, "was administered under very exceptional conditions in 1870 and in 1871; and yet the reception of patients [at the Bureau d'Admission à l'Asile Sainte-Anne] was never interrupted, not even during the times of the greatest excitement. Notwithstanding the disastrous events of which Paris was the theatre during that dreadful period, means were found to satisfy all requirements by adopting a few unimportant modifications of the usual routine. The documents which indicate the movement of the asylum population would imperfectly disclose the actual state of the case if unaccompanied by a few words in explanation of the manner in which the service was carried on at that time.

"At the beginning of 1870, the insane persons sent to the central office by the prefecture of police, the commissaries of police, and the different hospitals, at first remained for some time in residence at the admission office, whence, according to the nature of their cases, they were sent to Sainte-Anne's Asylum, to the Ville-Evrard Asylum, the Vaucluse Asylum, and the different sections of the Salpêtrière and of Bicêtre. When invasion threatened, the Ville-Evrard Asylum, being exposed to the enemy's fire, was evacuated, its patients being transferred to the Vaucluse Asylum. When the investment of Paris was accomplished, the Vaucluse Asylum was in a state of complete isolation. At that date, the sections of the Bicêtre were likewise evacuated; and then, also, the reception of insane women was discontinued at the Salpêtrière. From that time, during the whole period of the siege, Sainte-Anne's was the only asylum open for the reception of persons stricken with insanity in a population of at least two millions. Anxious to have a certain amount of accommodation available for new cases, the administration adopted a plan calculated to protect the interests of the patients and of their families. On the approach of the Prussians, a certain number of the insane were transferred to the different asylums in which boarders were maintained by the department of the Seine—the asylums of Armentières, Saint-Venant, Bourg, Bassens, Auch, Sainte-Gemmes, and Evreux. Events followed one another with such rapidity that the persons employed to carry out the last translations had difficulty in getting back into Paris: on their return, the Prussians were scouring the suburbs, and had cut off the means of communicating with the capital. The insane patients sent into the country escaped the emotional influences of the siege, the dangers of the bombardment, and, in a special manner, the privations and all the other pernicious influences which caused so great a mortality among our patients.

"On the 10th September 1876, in consequence of the departure of 480 insane, the population of Sainte-Anne's was reduced to 138, of whom 72 were men, and 66 were women. Although deaths were numerous, the available accommodation was filled up by new cases during October, November, and December.

"On the 20th January Sainte-Anne's contained 613 patients, of whom 321 were men and 292 were women.

"At that active period of the bombardment the lives of the insane were exposed to danger at every moment. The district of the town in which the asylum is situated was one of those which suffered most; and, among the establishments for the sick which seemed to serve as an objective for the Prussian shells, Sainte-Anne's occupied a first place. Precautionary measures were at once taken: as the violence of the enemy's fire was redoubled by night, the patients most exposed were taken down to the cellars at nightfall, and 108 uncured were allowed to go to their homes, of whom 44 were men and 64 were women. There then remained in the institution 262 men and 229 women.

"The prudence of the measures which were taken deserve praise. Notwithstanding the great number of projectiles which fell within the precincts of the hospital, and the large number of people collected in a relatively small space, we had to deplore only one patient killed.

"At the termination of the war, when there had hardly been time to attend to the most urgent requirements of the situation, the Commune, by taking possession of Paris, exposed our service to new trials. It was then no longer sufficient to provide for the material wants and the board of numerous patients, as in the general hospitals: it was likewise requisite to protect the large number of insane persons sequestrated under laws not understood by a power which was guided by no other principle than its own unrestrained will. The wards of MM. Lucas and Dagonet continued to receive all the insane patients in March, April, and May, 1871. To avoid disputes, it was sometimes necessary to alter the mode of admission. One day the mob wished to force the gate to obtain an admission in an alleged case of urgency; and, on another occasion, the interference was by a club which desired to decide by its vote as to the legality of shutting up persons. Those pretensions, however, did not lead to the serious consequences which might have been feared. During the street-fighting the asylum was simultaneously exposed, for several days, to the opposing fire of the army and of the national guards. One male attendant only was wounded. Some of the buildings were more or less injured. These preliminary observations may be useful, at some future period, to those wishing to study thoroughly the manner in which the insane were taken care of during the events of 1870 and 1871.

"The insane of the department of the Seine are under different conditions from the insane of provincial asylums. The latter remain in the asylums in which they are received, from the time of their admission till their recovery or death. Some of the insane of the department of the Seine, in addition to many other special conditions prior to admission, have, during later years, been frequently passing from one asylum to another: when the different services became crowded, a selection has been made from the chronic cases of the patients whose state permitted of their removal; and, at present, more than three thousand of this description are distributed among different public and private provincial asylums.

"On examining the record of admissions at Paris, since 1801, we find that, for a long period, with rare exceptions, more women than men were received. Since 1863 there has been a change in this proportion. In the ten years from 1801 to 1840, the total number of women exceeded the total number of men admitted. After 1841 there is given an annual numeration by sexes. From 1841 to 1863, the number of female is generally in excess of the number of male admissions. In three years there was a very slight exceptional difference, viz.:—

| Years. | Men. | Women. |
|---|---|---|
| 1843 | 668 | 667 |
| 1845 | 618 | 609 |
| 1859 | 977 | 946 |

"The new period in which male are regularly in excess of female admissions begins in 1863. It is thus represented:—

| Years. | Men. | Women. |
|---|---|---|
| 1863 | 1046 | 999 |
| 1864 | 1057 | 1026 |
| 1865 | 1203 | 1045 |
| 1866 | 1297 | 1148 |

"The above figures are all taken from M. Husson's reports.

"From the 1st January 1867, the date of the opening of the Admission Office, the numbers stand as follow:—

| | Years. | Men. | Women. |
|---|---|---|---|
| 1st May to 31st Dec. | 1867 | 834 | 747 |
| | 1868 | 1397 | 1188 |
| | 1869 | 1349 | 1083 |
| | 1870 | 1460 | 1060 |
| | 1871 | 1125 | 1073 |

"These figures are taken from the official register of admissions; but they differ from those furnished by the Assistance Publique, which, besides admissions to the asylums of the Seine, include natives of that department placed under confinement in different provincial institutions.

"It will be observed that, since 1841, the number of male admissions has pretty steadily increased, except during the years 1849, 1853, and 1854, when there was a slight tendency in the other direction; and, in the year 1870, when there was a notable decrease.

"There has always been a desire to discover the influence of political events in the production of insanity: facts do not appear to confirm the generally-accepted opinion that periods of social excitement greatly increase the admissions to lunatic asylums. Marcé has said:—'Times of revolution only morbidly exalt and lead to insanity those already predisposed, and who would probably have become insane under the influence of some other immediate exciting cause. The delirium may borrow much of its character from the political events of the day; but there is no appreciable increase in the number of the insane during revolutionary periods.' Statistics confirm this view. It is sufficient to examine the subjoined statement of the number of admissions for a certain period in the department of the Seine.

| Years. | Admissions. | Years. | Admissions. |
|---|---|---|---|
| 1847 . . | 1230 | 1853 . . | 1399 |
| 1848 . . | 1318 | 1854 . . | 1493 |
| 1849 . . | 1351 | 1868 . . | 2009 |
| 1850 . . | 1245 | 1869 . . | 2432 |
| 1851 . . | 1334 | 1870 . . | 2519 |
| 1852 . . | 1527 | 1871 . . | 2198 |

"These figures show that years of political agitation do not seem to yield a relative increase in the number of cases of insanity. The succeeding period is sometimes marked by a diminution in male admissions. At least this occurred in 1850 and at the end of 1871. Many persons of unsound mind took part in the popular movements: the victims of their own morbid excitement, they found a suitable aliment for it in the surrounding commotion. During the contest many of these persons lost their lives through their temerity and want of perception of danger; and when it was over, the surviving insane persons who had participated in it were sent with others, accounted responsible for their actions, to the prisons, the hulks, or the penal settlements. The judges, being unacquainted with medical science, could not distinguish the subjects of a cerebral lesion from other accused individuals able to estimate the nature of their actions."—P. 183.

Many interesting statistical expositions follow the passage now quoted. For them, and for discussions which we have either not touched upon, or only slightly noticed, we refer to Dr Magnan's volume, which is one of the most valuable recent additions to the literature of the pathological physiology of the nervous system. The author shows himself to be a man of the highest capacity as a clinical observer, as an experimentalist, as a thinker, and as a reasoner. He draws conclusions with an admixture of acumen and caution which peculiarly commands the confidence of his reader.—J. R. C.

*Sir William Fergusson, Bart.: A Biographical Sketch.* By Henry Smith, Surgeon to King's College Hospital. London: J. and A. Churchill, New Burlington Street: 1877.

We have read this little work with much interest and pleasure. Written by a friend, an assistant and colleague, who had the best opportunities of knowing him as a man and watching his distinguished career as a surgeon, it must be very gratifying to the relatives and friends of the deceased. Mr Smith is himself an accomplished surgeon, and can therefore speak with intelligent appreciation. He has come to the same conclusion, with every one who has in any way spoken of the personal and professional merits of Sir William, that his capabilities and performances as a surgeon were equalled by his unselfishness, his consideration for others, the staunchness of his friendship, and his readiness to forgive and forget injuries. The following anecdote gives a touching view of his character:—"The success of others was a source of great pleasure to him; and the very last visit he paid to the theatre of King's College Hospital illustrated this. Although worn down by his wasting disease, he, on hearing that his old assistant, William Rose—who had been recently made assistant-surgeon to the hospital—was about to perform his first case of lithotomy there, determined to come down and take charge of the staff. Rose performed the operation in a style worthy of his old teacher, who, on the completion of the operation, turned round to me and said:—'It is a great treat, after being away so long, to come to the old place and see lithotomy performed in such style.'" We advise our readers to procure the sketch, and treasure it in their minds.

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*The Vivisection Question Popularly Discussed.* By Dr L. Hermann, Professor of Physiology in the University of Zurich. Translated and Edited by Archibald Dickson, M.D. Williams and Norgate: 1877.

Dr Dickson has conferred a substantial benefit on the British public by this well-timed translation of Dr Hermann's pamphlet. The old expression—"Done into English from the original German,"—which always conveyed a suspicion of clumsiness in the execution, is quite inapplicable on the present occasion, because Dr Dickson's translation is thoroughly idiomatic, and evidently the work of one who is master of his native language. He introduces it with a thoughtful and tersely expressed preface, and has bestowed great pains in editing the text and illustrating it with appropriate annotations. In these latter, he has availed himself of some luminous passages from the speeches delivered in Parliament by Mr Lyon

Playfair and Mr Robert Lowe, who there brought their clear intellects to bear in a *trenchant* style upon a subject little understood and much misrepresented.

Several things must strike the reader of this pamphlet. 1. He will discover the foregone conclusion at which all those who oppose experiments on living animals, for the advancement of physiological science, seem to have arrived, that medical men, as a body, are hard-hearted and regardless of suffering. We have always believed the very reverse to be true, and cannot therefore but feel indignant at every proposal to check their supposed cruel tendencies by legislative enactments.

2. The bluebook published by a Royal Commission on the subject is remarkable for the strong testimony borne to the importance of experiments on living animals by men of the highest standing in their profession, and noted for their calmness of mind, their candour, and their benevolence. Among these we find the honoured names of Thomas Watson, James Paget, George Burrows, William Gull, William Jenner, and others well entitled to speak.

3. Without attaching undue weight to the *ad hominem* and *tu quoque* form of argument, we cannot but notice the palpable inconsistency in our laws taking no cognisance of the sufferings habitually inflicted by certain classes of society in the pursuit of mere amusement, and by farmers and others for economic ends and purposes, when a *comparatively* trivial amount of animal suffering is sternly interdicted in the case of a mere handful of scientific men, anxious to extend the domain of Physiology with all its magnificent possibilities in the time to come. The experiments performed by these men—mostly with anæsthetic appliances—are altogether trivial, when we think of the sufferings necessarily endured by poor hunted creatures in the chase, by wounded birds at *battues* for idle amusement; and the hundreds of thousands, every year, of horses, cattle, sheep, swine, and domestic fowls, which are subjected to a severe operation, that the convenience of individuals may be secured, and the animal food of the country rendered more abundant, more edible, more luxurious. Surely there is something unjust and absurd in legislating against the educated and earnest votaries of science, when other parties, especially the votaries of pleasure, are let alone?

4. The new enactments, while insulting and vexatious, are, at the same time, virtually impotent. By what system of espionage are they to be carried into effect? Is it desirable, that men of education and culture, animated with zeal for the extension of a science so important in its relations to human welfare, should be exposed to the temptation of doing what they feel to be a duty in the face of unreasonable restrictions and penalties?

5. It is provoking to read the criticisms of this worthy Swiss Professor on the recent legislation of our country; and all the more provoking that they are, on the whole, just and true.

The fact is, that the Legislature, exhausted by the labours of a tedious session, in the sultry evenings of August 1876, were unduly tempted to hasten through and pass a crude and imperfectly considered measure. There had been a noisy, nervous, almost hysterical outcry for many months, throughout the country—raised, in the first instance, by benevolent, well-meaning people who knew little about the subject by previous education, and then sustained and dunned into the public ear by paid officials of a society, we know with what result.

We believe that the Act so recently passed might be advantageously repealed. It can only vex and annoy, without really preventing the scientific investigations which its promoters had in their eye; and if any carelessness or indifference to animal suffering did exist previously to the ferment which has been excited, we venture to predict that such sentiments will now be reduced to a minimum.

We cordially recommend this little work so well introduced by Dr Dickson. It is full of reliable information, and places the whole subject of vivisection (an unfortunate word, fitted to excite popular prejudice) in a reasonable point of view.

### *A Text-Book of Physiology.* By M. Foster, M.A., M.D., F.R.S. Macmillan & Co: 1877.

Several years ago the English student of medicine was but badly provided with text-books on physiology, his choice generally lying between the thick and ponderous tome of Carpenter and the various editions of Kirke. There is no more hopeful sign of the impetus which the study of physiology has received in this country than the increasing demand for a good text-book on this subject. The demand indeed was so urgent, that the superior excellences of Hermann's "Grundzüge" led Professor Gamgee to translate it, and thus bring it within the reach of the English student of medicine. We have had occasion to notice the various text-books as they have been issued, and now we are called upon to notice another by one of our most distinguished English physiologists.

The author, in his preface, points out that the main object of writing this book was "a desire to contribute to the development of physiology in the medical profession." He has been actuated by the principle of giving critical expositions of the respective values of things, new and old, tumbled out of the physiological treasury; for, as he well remarks, the labours of the Physiological Laboratory at Hinauf seem to the student to be chiefly directed to contradicting the results of the Laboratory of Hinab. This tendency was expressed in rather a different way by another English physiologist, who maintained that the labours of Herr Langkopf were chiefly directed to contradicting those of Herr Breitkopf.

In the introduction, the fundamental vital properties of the lowly Amœba are considered under the following heads:—It is contractile, irritable, and automatic; receptive and assimilative; metabolic and secretory; respiratory and reproductive; and following these various functions, he advocates a physiological classification of the tissues. To these in the higher animals a vascular bond and co-ordinating nervous system have to be added.

The body of the work is divided into "Books," each of which is again subdivided into chapters. Large and small print are employed according to the importance of the subject, the student being advised to omit the matter in small type at the first reading. It may be mentioned at once that this book contains no histology, but is devoted entirely to pure physiology; and it contains but few diagrams, a want which may, and does, render some of it rather hard for the student to comprehend. The first chapter treats of the blood—its chemistry, and the cause of its coagulation. The view of Schmidt is followed, that "Coagulation is the result of the interaction of two bodies, fibrino-plastin and fibrinogen, brought about by the agency of a third body, fibrin-fibrin." The recent researches of the same investigator on the action of dilute NaCl and hæmoglobin on coagulation are noted; and, indeed, the most recent researches of Schmidt, contained in the latest numbers of *Pflüger's Archiv*, are embodied in the text. Perhaps it might have been well, both in relation to this subject and several others in this work, had many very recent statements not been so rapidly incorporated with that knowledge which a student is expected to possess, for, as Morrant Baker shrewdly remarked, many statements gravely given forth as facts do not withstand the allotted three years' test. The views of Lister and Brücke as to the cause of the blood remaining fluid in the vessels are critically given, and the importance of the colourless corpuscles as a source of the fibrin ferment is pointed out.

The second chapter treats of the contractile tissues—the chemistry of muscle being taken first, and it is suggested that though living muscle contains no myosin, "it contains some substance or substances which bear somewhat the same relation to myosin that fibrinogen and fibrino-plastin do to fibrin, and which become myosin at the death of the muscle." The phenomena of muscle and nerve are then discussed, though we think the arrangement might be improved by omitting the consideration of the electrical currents in nerve till a later date, for this is always a matter of considerable difficulty to the student. The exposition of Bernard's urari or curara experiment, showing the independence of muscular contractility is very clearly given, though we have failed to notice the modification of this experiment suggested by Rosenthal. Very few and imperfect descriptions of instruments are given, and only the principle of the pendulum myograph of Fick is indicated; indeed, the want of diagrams makes the description of all such instruments a very difficult task. The apparatus of Bernstein for the study of the negative variation passing over any given spot on a nerve, is complicated enough even with a diagram, and is rendered doubly so by the want of one.

The fundamental properties of the nervous tissues are then treated of, and we have some very good remarks on inhibition, reflex action, and automatic action, the difference between the latter two being carefully pointed out and maintained both in this and subsequent chapters.

The fourth and last chapter of this book is devoted to the vascular mechanism. A few tracings of blood-curves are given, and it would have been better had the tracings been so taken as to be read off always in one direction, which would have avoided the necessity of stating each time whether they are to be read from right to left, or left to right. An example of the difficulty a student might meet with in understanding a description of an instrument without a diagram is met with at page 90, in the case of the Stromuhr,—not a very reliable or very useful piece of apparatus. The section on the hydraulic principles of the circulation is very good; the differences between the flow of a fluid through rigid and through elastic tubes being clearly indicated; and all the remarks on arterial tension will be found of value, it being specially insisted upon that the arterial system is *overfull*. But the author is not led away by mere mechanical principles, for, later on, he is most careful to consider the vascular system in its relation to the nervous mechanism governing it, in a manner at once agreeable and interesting to the reader. We are glad to observe that due weight is given to the researches of Ceradini on the mechanism of the semilunar valves of the heart, and the "Selbststewgerung" theory of Brücke is fairly discussed. The second sound is rightly attributed to the sudden *tension* of the aortic and pulmonary semilunar valves. Doubts are thrown upon the muscular origin of the first sound, notwithstanding the direct experiments of Ludwig and Dogiel.

The medical student, in a text-book of physiology, has, we think, a right to expect a description of the sphygmograph, for it is one of the few physiological instruments directly available in practice; but no description of it is given. The relations of the heart with the cerebro-spinal axis, and the physiology of each nerve connected directly or indirectly with the heart, are discussed in detail. The effects also of some poisons on the heart are given. The position of the vaso-motor centre, as indicated by Owsjannikow, is given, though Dittmar has recently defined its position in the medulla more accurately than the Russian observer. We regard the view of this centre taken by the author as the correct one; and the local vaso-motor mechanisms, long ago suspected by Lister, and lately brought into more prominent notice by the experiments of Goltz on the spinal cord, come in for a very fair share of discussion. This chapter closes with a synopsis of the mutual relations and the co-ordination of the vascular factors.

The second book treats of the tissues of chemical action with their respective mechanisms, and of nutrition. The first chapter is devoted to digestion. This is in many respects an excellent *resumé*, embodying, as it does, most of the recent views on this subject, including those of Kühne, though we think there is here also the tendency to admit the most recent statements, some of which may be found to be premature. In the chemical part of this chapter the knowledge is up to date, and assimilated more to what is to be found in the text-book of Gorup-Besanez, a book with which many of our English physiologists seem to be too little familiar. The summing-up of our knowledge on the ferments is good, and the distinctions and differences between pancreatic and peptic digestion are more clearly brought out than in any other English book. The whole physiology of the secretion of saliva is excellently put. The quantity of gastric juice secreted has always been a difficulty; but surely the amount given by the author will be sufficient to cover all difficulties, viz., 13–14 litres in 24 hours. The recent researches of Heidenham on zymogen, and on the outer and inner zones in the cells of the pancreas, find a place.

In speaking of the pressure under which the bile is secreted, it is given in the guinea-pig as 200 mm. of Hg, and the quantity in man secreted in 24 hours is given as 10 kilos; the secretion of bile is known to be suppressed by a very few mm. of Hg, and, therefore, we are at a loss to know how this 200 mm. has been obtained. Howalewsky found the pressure under which bile could be secreted in the cat to vary from 12–20 mm. Hg.

At p. 189 an attempt is made to reconcile the conflicting statements regarding the digestive properties of the mucous membrane of the pyloric end of the stomach. In discussing the muscles of deglutition, it would have been better had the nerve of supply to each muscle been given more precisely. The vertical lines to be observed on the clear hem of the epithelial cells covering the villi of the small intestine, the author does not regard as pores, as imagined by Kölliker.

The tissues and mechanism of respiration are treated of in the next chapter. This is a very good part; and the section on hæmoglobin, its properties and its derivatives, is one of the best in the book. The influence of the nervous system on respiration is carefully discussed, and it is concluded that the action of the respiratory centre is automatic. Chapter III. is devoted to secretion by the skin; and IV., to that by the kidneys. At p. 281 there is a statement regarding the effect of constricted and dilated cutaneous vessels on the secretion of urine, which puzzles us.

We now come to the metabolic tissues of the body, and to nutrition in general. The whole history of the glycogen question is excellent, the author holding that the older view of Bernard has not as yet been clearly disproved. Some very hypothetical suggestions as to the sources of urea in the economy are put forward.

The central nervous system and its instruments occupy the third book, which opens with a discussion on sensory nerves, the development of special end-organs, and the evolution of certain groups of nerve-cells connected therewith, and placed in the central nervous system in connexion with the so-called organs of sense.

All the special senses are confessedly very briefly dealt with, sight being the first one treated of and most attended to. We really had expected to see something about the ophthalmoscope and the examination of the erect and inverted image on the retina, but we looked in vain. How the size of Sanson's images is to be measured, viz., by the ophthalmometer, does not find a place. We observe that the names of Dewar and M'Kendrick are mentioned in discussing the effects of light upon the electrical currents of the retina, but Du Bois Reymond was the first to touch this subject, and Holmgren of Upsala had already written largely upon the matter before either of the two previous observers so successfully attacked the subject. To the sense of hearing but nine pages are devoted. Smell, taste, touch, and feeling, and the muscular sense closes the chapters on the special senses. These subjects are all rapidly passed under review.

The nervous system is then attacked, and first the spinal cord is treated of as a centre of reflex action. The arrangement followed in discussing reflex action is a very good one. The frog and the mammal differing so markedly in respect of their reflex spinal phenomena are considered separately. The salient features of an ordinary reflex action in the frog are discussed, and also the circumstances which modify this combination of processes. The phenomenon of inhibition of reflex action is gone into fully both as it occurs in the frog and mammal. As to the paths of conduction of impulses in the cord, we have the recently published views of Woroschiloff incorporated in the text. Altogether, the chapters on the spinal cord are very good.

The physiology of the brain is entered into plainly *con amore*. Against the view that the cerebral cortex contains "centres" the author adduces, we think, good reasons why this view should not be accepted so readily as some have done. References are given to most of the newer investigations on this matter. The section on the cranial nerves, we think, is far too meagre and not well suited for the wants of a student, for he does not like to hunt through a book to get that information which could be easily formulated for its more easy acquisition. The final chapters on special muscular movements, the tissues and mechanisms of reproduction, menstruation, impregnation, nutrition of the embryo, parturition, and death, are all short, and the development of the embryo is omitted intentionally.

There is an appendix drawn up by Mr N. S. Lea. It treats of the chemical basis of the body.

To each book there is added a short list of the classic authors and their chief publications. Scattered throughout the text are references to most of the newest publications on particular subjects. We think that the practitioner will find this work a great assistance to the better interpretation of many pathological phenomena meeting him in daily practice. The author has apparently been guided throughout in the production of this work by the true scientific spirit, and though in some respects it is not all that could be desired to supply the wants of a student of medicine, still it contains much that must stimulate thought, and thus lead to the more vital and earnest interest of the medical profession in the too much neglected department of physiology.

# Part Third.

## MEETINGS OF SOCIETIES.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION LVI.—MEETING VII.

*Wednesday, 4th April* 1877.—Dr J. D. Gillespie, *President, in the Chair.*

### EXHIBITION OF PATIENTS, PATHOLOGICAL SPECIMENS, ETC.

I. *Mr Annandale* showed—(1.) A young man whose case illustrated the value of the antiseptic treatment in connexion with conservative surgery. Some time ago the patient's arm had been drawn between two hot calendars, by which the soft parts were greatly lacerated and the elbow-joint opened. When he came under his care, there was osseous anchylosis at the elbow, and one long cicatrix on the front of the forearm, causing extreme flexion of the wrist and fingers. He first excised the joint, removing about six inches of the bone in the hope of making a movable joint, and also relieving the flexed fingers. Finding that the excision of the elbow did not relieve the flexion of the hand, he dissected up the cicatrix, thus making a very large wound. In the after-treatment extension was applied to the fingers by means of a weight. As they now saw, he had perfect use of the elbow-joint and a useful hand. (2.) A man whose case was unique, he believed. He had a large reducible femoral hernia of the size of a closed fist, which came down so constantly that he was unable to follow his occupation. He performed the operation already described,[1] by making an incision somewhat above and parallel to Poupart's ligament, so as to reach the neck of the sac and ligature it with catgut. The operation was performed about four months ago, and since that the patient had been going about without any truss or band.

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[1] Edin. Med. Jour. for June 1876, vol. xxi. p. 1088.

This was one of the few cases known in which a successful radical cure of a reducible femoral hernia had been accomplished. (3.) A YOUNG WOMAN whom he had previously exhibited, as showing the result of the excision of the joint between the os calcis and astragalus. They could now see the excellent general result and the almost perfect movement at the ankle-joint.

II. *Dr Argyll Robertson* wished to show a PATIENT SUFFERING FROM MINER'S NYSTAGMUS, as Dr Bramwell had mentioned to him that he intended exhibiting a similar case to-night. His own patient had been previously under the care of Professor Grainger Stewart. It differed from ordinary cases, inasmuch as where the patient looks at an object on the horizontal plane or slightly below it, the eyes are fixed; whereas when the object is above the horizontal plane, oscillation of the eyeballs takes place. Another peculiarity was that jerking of the head also caused the oscillation. He attributed the movements brought about when the object was above the horizontal plane to the more rapid fatigue caused by the unusual effort necessary for the convergence of the optic axis and the change in accommodation. He thus thought that if he could stimulate the power of accommodation he might remove one of the leading causes of this unusual fatigue. He had, therefore, been trying the application of a single drop of the active principle of the Calabar-bean; and so far as he could judge from the limited trial made, there had been some slight improvement.

III. *Dr Byrom Bramwell* showed a CASE OF MINER'S NYSTAGMUS, and said, "After the interesting remarks of Dr Argyll Robertson, it is unnecessary for me to say much in introducing to your notice a second case of miner's nystagmus. The patient, aged 49, was admitted to the Newcastle Infirmary, under my care, on 31st January of the present year, complaining of dimness of vision and oscillation of the eyeballs. The trouble was of six months' duration. It commenced gradually, without any apparent cause. Eighteen months before the date of his admission, he commenced to use the Davy lamp; but, before that, he worked with a candle. He thinks the glare of the lamp hurt his eyes. The oscillation is, you will see, very marked. It is increased by any sudden movement, when he becomes excited, and is always worse in the dark. His general health is, and always has been, good. The fundus, as seen by the ophthalmoscope, is healthy. This is the fifth case of this disease which I have met with since the appearance of Dr Charles Bell Taylor's paper (*Lancet*, 12th June 1875). I have not found the disease so curable as that writer states."

IV. *Mr Hamilton* showed several MICROSCOPIC PREPARATIONS ILLUSTRATIVE OF VARIOUS DISEASED CONDITIONS.

*Arteritis Obliterans.*—The preparation was from one of the arteries at the base of the brain in a male patient who suffered from constitutional syphilis. The case was that recently communicated to the Pathological Society of London by Dr Davidson in the discussion on visceral syphilis. Staining in perosmic acid brought out the appearances better than any other re-agent. The lesion was identical with that described by Heubner and Friedländer, consisting in an inflammatory thickening of the intima in which bloodvessels were abundantly developed. The new formation, consequently, never underwent atheromatous degeneration. The thickening of the intima, however, was somewhat peculiar in this instance, in so far as the material which produced the thickening was arranged in several successive layers. This appearance was pointed out by Dr Greenfield when the case was presented to the Pathological Society; but he only referred to two, whereas, in some of the smaller branches of the vessels there were as many as three or four newly-formed rings. The inner layers did not seem to be outgrowths from the endothelium, but were evidently due to inflammatory hyperplasia of the intima itself. They were, presumably, more recent, and hence much less organized deposits than the outer, which were fibrous in their nature. The inner elastic lamina and muscularis were almost quite unaltered, but in some parts of the adventitia there was a very abundant cellular infiltration. The vessels, to the naked eye, were thickened, their lumina very small, and the coats had a grey gelatinous consistence. In no place were yellow atheromatous patches found. At certain intervals in their course there were little nodular swellings, probably accounted for by the infiltration here and there of the adventitia.

*Channel Polypus.*—The preparation was procured through the kindness of Dr Underhill. It was removed from the interior of the uterus. When first detached it presented an extremely soft velvety surface, from which numbers of channels filled with mucus, and of the calibre of a crow-quill, ran into the substance of the tumour. It measured about two inches in length. Sections showed that the basis of the tumour consisted of a fibro-cellular material in which were numerous bloodvessels. The channels were in all probability enormously dilated follicles of the uterine mucous membrane, as they, in every instance, were lined with a perfect layer of columnar and ciliated epithelium, and could be seen of normal size and in all stages of dilatation. The tumour was evidently similar to that described by Dr Oldham in Guy's Hospital Reports.

*Myeloid Sarcoma of Femur.*—The tumour was an extremely typical instance of a myeloid sarcoma. It was almost entirely composed of giant-cells, of enormous size, containing thirty to one hundred round or oval-shaped nuclei. These were imbedded in a matrix composed of granular material, bloodvessels, and small round sarcomatous cells.

*Small Round-Celled Sarcoma.*—This new formation was removed from the tongue of a male patient, was about the size of a small walnut, and of fleshy consistence. It was made up of comparatively small round cells, highly nucleated, and infiltrating the surrounding muscular tissue.

*Miliary Aneurisms of the Brain.*—These were two in number, and were removed from the brain of a girl, aged eighteen, who died from cerebellar hæmorrhage. They lay quite superficially over one of the corpora striata. They were fusiform in shape, and were distended with blood. The vessels generally, throughout the brain, even to the minutest capillaries, were in an advanced state of fatty degeneration.

V. *Dr Byrom Bramwell* showed the following PATHOLOGICAL SPECIMENS :—

(1.) *A Malformed Heart.*—There were only two segments to the aortic valve, the third being represented by a depression situated below and between the two large ones. There was only one coronary artery. A polypoid vegetation $\frac{3}{8}$ths of an inch in length was attached to the free margin of one of the large segments. The heart closely resembled Fig. 3 in Dr Peacock's 8th plate. (*Malformation of the Heart.*)—The patient, a man æt. 46, was admitted to the Newcastle Infirmary, under Dr Bramwell's care, on 26th February 1877, and suffering from Bright's disease. The heart was somewhat hypertrophied (its weight after death being 1 lb. 2 oz.) There was a loud double aortic murmur and a well-marked Corrigan's pulse. The patient died from uræmia on 15th March.

(2.) *An Atrophied Heart.*—The patient, a female æt. 45, married, the mother of several children, died after a lingering illness, under Dr Bramwell's care, from cancer of the pylorus. The body was literally reduced to a skeleton. The heart weighed only 2 oz. 12 drs. 11 grs. (avoirdupois). When in health the patient was well nourished, though never fat. Her height was 5 feet 4 inches.

(3.) *A Tumour on the Exterior of the Heart.*—The tumour was the size of a hen's egg, and was situated on the exterior of the right ventricle. The patient, a female æt. 32, died suddenly a few days after delivery. The chest only was examined. Nothing abnormal was found except the cardiac tumour. The heart itself was healthy. The patient had suffered for some years from dyspnœa and palpitation. On microscopical examination, the tumour was found to consist of cells, bloodvessels, and fibrous tissue. The cells, which were round and angular in shape, generally contained a well-marked nucleus. The cells were about twice the size of a white blood-corpuscle.

VI. *Mr Annandale* showed a PATHOLOGICAL SPECIMEN, rare so far as he knew. It was from a case of injury to the elbow of one year's standing in which he had excised the joint for anchylosis. The operation was difficult, and, when finished, the following was found to be the state of matters: The humerus had been fractured above the condyles, and the upper fragment driven down and anchylosed to the front of the radius and ulna by bony union. It was interesting that such a firm union should take place between smooth surfaces of bone.

VII. *Mr Annandale* then read a paper on THE OPERATIVE TREATMENT OF INTERNAL HÆMORRHOIDS, which appears at page 1077.

The *President* said that the Society would be glad to hear any remarks on Mr Annandale's admirable paper.

*Dr John Duncan* thought that the members were indebted to Mr Annandale for bringing forward a question which had not, as yet, been much discussed in this School, as they had been long wedded to the use of the ligature. Hitherto, he had been content with the ligature; but he believed it would be useful to experiment with the plan detailed. He could hardly, however, come to Mr Annandale's conclusion, as it was not a question of the value of the cautery-wound and wounds in general, but of the cautery *versus* the ligature-wound. He believed that the cautery caused less pain than the ligature; but, by many good surgeons, cautery-wounds, like burns, were found more liable to be followed by secondary hæmorrhage when the slough separated. He did not believe that it would be ultimately shown that there would be less risk of pyæmia in the case of vascular tissues when the clamp and cautery were used, than when the ligature was employed. Both occluded the vessels for a time; and, in both, there was a risk of pyæmia coming on later.

*Dr Cadell* could endorse Mr Annandale's conclusions from his personal experience and from his having seen Mr Smith operate. The clamp and cautery was quite as efficient as the ligature, cured more rapidly, and was a less painful operation. He had seen Mr Smith operate without chloroform by means of the clamp and cautery on an Indian officer who previously had his piles removed by ligature and also without chloroform. This patient's experience as to the pain felt was that there was much less with the clamp and cautery. Indeed, pain was only felt when the heat was transmitted through the clamp to the surrounding tissues. Sometimes it was difficult to apply the clamp when the pile was sessile. He was in favour of the clamp and cautery.

*Mr Chiene* had tried the clamp and cautery after Dr Cadell had told him of Mr Smith's success and from what he had seen of Mr Annandale's practice. He was satisfied that, with the clamp and cautery, there was less pain at the operation and when the bowels were moved. He had not yet had experience on the point of pyæmia, and he could not bear out Mr Annandale's remarks as to the speedy healing. The patients might be out of bed, but he would like to know if they were not walking about with unhealed ulcers in the rectum.

The *President* thought it probable that in regard to the time of healing, the clamp and cautery gained a certain number of days, viz., the seven or eight days the slough caused by the ligature took to separate. He did not think sufficient stress had been laid by Mr Annandale in his paper on the fact that operation was often employed when constitutional treatment might have been effectual, as in cases where the pile was so small that it could be laid hold of with difficulty. In some cases when asked to operate he had found mild laxatives of great use. From what he had seen he was inclined to favour the clamp and cautery, as being more rapid and less liable to be followed by pyæmia. He had seen pyæmia follow ligature, but we had not yet a sufficient number of cases recorded of treatment by the clamp and cautery to enable us to judge in which method of treatment pyæmia was most frequent.

*Dr Bruce*, as a general practitioner, had not much experience in operating on piles, although often consulted about them. He had tried nitric acid in one case with a most unsatisfactory result. He had seen cases where the ligature was employed, followed by a great deal of pain and strangury. Presuming that it was an equally safe operation, both as regards pyæmia and secondary hæmorrhage, he would recommend his patients to have their piles removed by the clamp and cautery rather than by the ligature. With regard to these points, however, it would appear that further experience was required.

*Dr James Carmichael* had often been astonished at the rapidity with which large piles in pregnant women disappeared after delivery. He would like to know if Mr Annandale ever operated on women within a few weeks after delivery, becaused he believed such interference could hardly ever be necessary, as almost invariably the largest masses disappeared in time with simple treatment, much more readily than piles of the same size would disappear in the male.

*Mr Bell* thought that all were indebted to Mr Annandale for his interesting paper, in which he had given a good series of cases where the clamp and cautery had been used. As to the time after the operation of ligature during which the patient was confined, his experience did not bear out what Mr Annandale had said. If the patient was well prepared the time was short. He himself had never had a bad case, but he had seen two, one in Mr Syme's practice. One point that struck him was, that success in using the clamp and cautery depended more on the dexterity of the operator than was the case with the ligature. He therefore thought that, as a general rule, the ligature would suit most people better than the clamp and cautery. Still in many cases the clamp and cautery proved valuable.

*Mr Annandale* had been much gratified by the kind discussion, and had only a few remarks to make on the criticisms. In reply to the President he might say, that from experience he was chary in operating until other treatment had failed. Dr Carmichael had spoken of piles in pregnancy. He never operated on such, and would not interfere after delivery unless other means failed. Dr John Duncan, whose experience of cauterized wounds was large, referred to hæmorrhage. A great deal depended on the heat at which the cautery was used. As to Mr Bell's criticism, he hoped that by publication of his paper a satisfactory answer would be got from the profession at large. Mr Chiene had referred to the question of the healing of the sore. He thought the President had answered that well. He himself did not think the escar from the cauterized surface took a week to separate. He had consulted several works, but found some difference of opinion as to the time of the separation of the ligature. From his own experience he thought ten to twelve days passed before the ligature came away when the piles were of any size. The after-pain was much less when the clamp and cautery was used, although he had had cases where both pain and strangury were experienced. If the President and members present had heard the expressions of some of his patients on the comparative painlessness of the clamp and cautery as compared with the ligature, he had no doubt they would also prefer it.

The *President* asked for precise information as to the heat of the cautery.

*Mr Annandale* said that the best plan was to make it red-hot, and then allow it to cool down to an almost black heat.

VIII. *Dr Cadell* then read a paper on THE VARIETIES OF INFECTING CHANCRE, which appears at page 1083.

The *President* invited remarks on Dr Cadell's paper.

*Dr John Duncan* imagined that one who had been for nine years an assistant-surgeon in the Infirmary, and in charge of the Lock Ward there, could hardly but have thought much on the subject of Dr Cadell's paper. Everything was not quite clear to himself, and therefore he might not be quite able to make it clear to them. He thought it good to take as broad a view of the subject as possible, and to begin at the beginning. Every living thing has its one typical form, and so it is with the manifestations of syphilis. The typical form of syphilis was, he believed, a circular neoplasm, modified by the tissues in which it may come about. In the skin, the manifestation has also its typical form, viz., a circular papule. Here, again, it is modified by locality, by intermingling with external irritants, by the constitutional condition of the recipient of the poison, and so on. So, in the primary manifestation of syphilis, there was a typical form, modified by locality. Applying these general observations, they might say syphilis was a single poison, and its first manifestation had a typical form modified by locality, viz., a circular papule modified into the shape of the Hunterian chancre in some cases, the desquamating nodule in others, or the chancroid erosion of Ricord when in mucous membrane. It was also modified by admixture with other poisons, or by the unhealthy constitution of those in whom it occurred. Indeed, it might be so modified that the type was not recognisable, as in phagedæna, although followed by constitutional disease. So far, he had said nothing that would be opposed by syphilographers; but, unfortunately, there was one modification causing a difference of opinion, arising from a difference in words, in fact, and in the interpretation of fact. As to the dualist theory being dead or alive, it was only a vague use of words. The dispute as to fact was from the occurrence of the soft chancre, *i.e.* a sore of a purulent character, immediate development, and power of autoinoculation, and was, Does syphilis follow a soft sore more frequently than it does a gonorrhœa? He thought it did. He did not forget the explanation given by Dr Cadell, that the two poisons were mingled, although he was not prepared to admit an invariable and palpable induration. But the question really was, Is this intermingling more frequent than other interminglings? Probably even dualists admitted this. If so, then why? The interpretation of facts was more difficult. Are we to explain it by supposing that the lymph of the soft chancre and hard chancre were more frequently mingled than that of the hard chancre and gonorrhœa purely by accident, or that the former two were in some way connected? One theory was that the syphilitic poison being mingled with inflammatory products gives a *tertium quid*,—the virus of the soft chancre. When potassium came in contact with iodine, they got a new substance, iodide of potassium. Mr Hutchinson's hypothesis, though rather vague, is similar to this. Practically when they saw certain sores, they could say this is syphilis; but with soft or phagedænic sores they could not say—they might or might not be syphilitic. Thus, as to fact and theory, they were still in the dark.

*Mr Chiene* said that the light he had gained from the remarks on Dr Cadell's paper was that they should have a special discussion on the subject. He agreed with much but not all that Dr Duncan had said. At the late hour, however, he would not prolong the discussion by any remarks of his own.

*Mr Annandale* suggested that they should wait, as the whole subject was in complete confusion. In some sores they could be sure that syphilis would follow; in others they had to give a very guarded prognosis.

The *President* had seen many cases of syphilis, and was much puzzled as to its nature. He could not give in his adhesion to the dual theory.

*Dr A. G. Miller* thought that much of the confusion arose from their confusing the virus with the chancre.

*Dr Cadell* replied, that the subject was so immense that it was too late to say much. He had never seen a soft chancre cause syphilis, nor a hard chancre with universal adenitis fail to do so.

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXXVI.—MEETING VIII.

*Wednesday, 14th March* 1877.—Professor Simpson, *President, in the Chair.*

I. *Dr Underhill* showed a FŒTUS PAPYRACEUS which had been sent to him some time since by a friend in England. It lay with the legs doubled up on the body, the feet reaching to the face. The body measured from the vertex to the buttocks 4½ inches, and from the vertex to the umbilicus 3½ inches. The full length would be about 8 inches. No history of the case was obtainable further than that it had been born twin with a well-developed full-grown child, which survived. The fœtus was squeezed to an unusual degree of thinness, and quite merited the descriptive title, "papyraceus." The spine and ribs were easily made out on the back aspect, and the navel, with three inches of cord still attached to it, on the front.

II. *Dr Ronaldson* showed a FŒTUS WITH INTRA-UTERINE PERITONITIS AND DILATATION OF BLADDER, UTERUS, AND KIDNEYS. One kidney was only partially developed. The child survived two days.

III. *Dr Macdonald* read a paper on TWO CASES OF PUERPERAL PLEUROPNEUMONIA, which appears at page 967.

*Professor Simpson* thought the Society was indebted to Dr Macdonald for this interesting communication. He had not met with a case parallel to either the one or the other of Dr Macdonald's cases. He had met, however, with cases of pneumonia after delivery resulting from infarcti. He also remembered a patient recovering well from bronchitis in whom, after delivery, symptoms of pneumonia came on, proving rapidly fatal; also, the case of a patient in the seventh month of pregnancy, with a double cardiac murmur, who suffered from slight broncho-pneumonia. She was delivered at the eighth month; double pneumonia supervened in the third week after confinement and proved rapidly fatal. Dr Macdonald's remarks regarding endo-cardial affection in these cases were interesting. He had no doubt that Dr Macdonald was correct in attributing the symptoms in these cases to endo-cardial lesion. Hæmic murmurs were not, however, uncommon in the puerperal state, and should not be confounded with these.

*Dr Ritchie* had seen a case, some years ago, very like Dr Macdonald's first case. She had repeated miscarriages resulting, in his opinion, from gonorrhœa contracted shortly after marriage. Phlegmasia came on, followed by pneumonia of right lung, which proved fatal. He had also seen several cases of intense bronchitis coming on shortly after delivery.

*Dr Thomson* had lately a case of pneumonia coming on seven days after delivery. Prune-juice expectoration was a marked sign in this case. The patient recovered.

*Dr Macdonald*, in reply, stated that he was much interested in the case Dr Thomson had mentioned. In regard to these cases, generally, he thought there was a certain individuality about them which would warrant us in regarding them as a special class of inflammations. There was apparently something peculiar in the inflammation occurring in certain cases of pneumonia of the lying-in period which distinguishes them from cases occurring at other times. He had watched, in his practice, cases of pneumonia in the lying-in period that ran their course as ordinary pneumonia apart from pregnancy. But what struck him in observing and thinking over the cases he had recorded to-night was the very many points of divergence between them and ordinary pneumonia, such as the rapidity with which the dulness was developed, its very absolute nature, the absence at first of crepitation, the largeness of it when it did form, and the bloody sputum. The most of them could be explained, however, on the embolic theory, if the other symptoms could be made to agree with it. His real difficulty was in finding an origin for the embolism and urgent symptoms to correspond with its period of occurrence. In all the labour cases he had attended for some time back, his attention had been directed to the state of the heart, and, in a certain proportion, he had found hæmic murmurs. In the cases detailed in this paper, the murmurs persisted after the patient had recovered, proving them not to be of this nature, but the result of organic lesion.

IV. *Dr Church* read NOTES OF A CASE OF HEMIPLEGIA IN A CHILD FOLLOWING APPLICATION OF THE FORCEPS AT BIRTH.

*Dr Craig* commended the paper and remarked that, although considerable injury was often done by use of forceps during delivery, yet permanent damage rarely resulted.

*Dr Macdonald* had met with a case in which the operation of perforation had been commenced. A strong pain came on expelling the child. No bad result ensued.

*Dr Ballantyne* believed that death in the fœtus was often the result of the use of the forceps. He referred to a case of Dr Sidey's, in which the head had been much indented after the use of the forceps, and the child died.

*Professor Simpson* mentioned a case which had been shown to the Society some years ago in which depression of the occiput had resulted from use of the perforator. The child recovered.

*Dr Ritchie* thought Dr Church's first explanation of the rationale of the case probably the correct one, that some apoplectic condition and not merely pressure was the cause of the hemiplegia.

*Dr James Carmichael* mentioned a case he had met with lately, and in which he had examined the head after the child's death. A great deal of capillary extravasation had taken place all over the cerebral hemispheres. In this case the labour had been quite natural, and the child was easily born without instrumental aid. He was at a loss to account for the extravasation of blood.

After a few remarks from *Dr Ronaldson*, *Dr Church* briefly thanked the Society for the reception of his paper, and the discussion closed.

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### SESSION XXXVI.—MEETING IX.

*28th March* 1877.—Professor SIMPSON, *President, in the Chair.*

I. *Professor Simpson* exhibited an acardiac FŒTUS, regarding which he proposed to give a full account, after dissection, at a future meeting.

II. *Dr James Cumming* read NOTE ON THE USE OF THE STETHOSCOPE IN OBSTETRICS, which appeared at p. 1002 of the Journal.

*Professor Simpson* thought Dr Cumming was correct in upholding the view of those observers who maintained that the uterine souffle originated in the bloodvessels of the uterus; but at what point in the course of the vessels the sound was produced, he believed was still undetermined, or whether, as Cazeaux thought, it might be due to the condition of the blood.

III. *Dr Wilson* read NOTES OF A CASE OF EXTRA-UTERINE PREGNANCY.

*Professor Simpson* regarded the case as an interesting addition to our record of cases of this description.

IV. *Dr Cuthill* read a paper on A CASE OF PATENT FORAMEN OVALE, OCCURRING IN LIVE BIRTH, in which respiration failed to be established, with PREPARATION OF THE HEART.

A short discussion, in which the *President, Dr Matthews Duncan*, and several of the Fellows took part, then followed *Dr Cuthill's* paper, after which—

V. *Professor Simpson* read for *Dr Paterson*, Bridge of Allan, NOTES OF A CASE OF ANENCEPHALOUS FŒTUS BORN CO-TWIN WITH A HEALTHY CHILD, WITH PREPARATION. Mrs S., married, æt. 32, strong and healthy, has had two children. Upon her third confinement she informed me that her labour had come on two months sooner than she expected. On the 16th of February I was called to attend her, and, upon examination, found the os uteri fully dilated and the vagina filled with the membranes. Labour pains not frequent, and weak. I ruptured the membranes, and the quantity of liquor amnii was very excessive. I collected a large chamberpot quite full, and there must have been lost in the bed an equal quantity. On further examination I was perfectly puzzled as to the nature of the presentation. Shortly, however, I found a small hand presenting. I then introduced my hand with a view to turning, and found a small deformed child, which was easily extracted. When born it was in a state of perfect rigidity. On placing my hand on the abdomen of the mother, I found another child *in utero*, which was born quite naturally one hour later, and evidently a seven-months' child. Both children were females; and it is a singular fact that Mrs S.'s mother was also delivered of twin daughters at her third confinement. Mrs S. states that during her pregnancy she felt quite well; but about six weeks before her confinement she had had a fall, but knows of no other circumstance likely to have produced premature labour. During my long experience this is the only anencephalous case I have met with.

*Professor Simpson* observed that the fœtus presented the usual characteristics. It was interesting that it had been born co-twin with a healthy child. The bag containing the anencephalous child contained great excess of liquor amnii. He referred to a paper he had formerly communicated to the Society which showed the frequency of hydramnios in these cases.

*Dr Jamieson* met in 1870 with two anencephalous cases in one week. In one case there was a large quantity of liquor amnii. The patient made a slow recovery, suffering from phlegmasia dolens. In the second case there was no excess of liquor amnii. In both cases the viscera protruded on account of deficiency of the anterior abdominal wall.

*Dr Bruce* had met with anencephalous cases, but never in twins.

*Dr Wilson* had also met with such cases. He referred to two of these. In one, there was great excess of liquor amnii; in the other, the mother had previously had a child with hare-lip.

VI. NOTES OF A CASE OF LABOUR COMPLICATED WITH EMPHYSEMA, by *E. Law, M.R.C.S.*, was then communicated by the *President*. A. M., æt. 26, primipara, a strong healthy woman, was admitted into hospital at 4 A.M., 23d February 1877. On examination immediately after, I found the os somewhat larger than half-a-crown, the membranes ruptured, and child's head in the first position of Naegele. She states that the labour had commenced about 11 o'clock the previous evening, and that the waters had broke about 1 o'clock. The pains were regular, and the second stage commenced towards 7 o'clock; all proceeded well for some time. At 12 o'clock the head was almost at the outlet, but had not rotated; it now made no advance, although the pains were very strong, and the bearing down and straining most severe. The caput succedaneum gradually increased and became very large. On examination at 3 o'clock I found the head had rotated and made some progress. Dr Ziegler, who had been summoned, arrived; and as the head was now making progress during each of the pains, which were not nearly so severe, he did not interfere; the labour terminating satisfactorily a little before 4 o'clock. Child, female, healthy; weight, 7 lbs. 6 oz.; length, 20 inches. Patient's strength good; made no complaints of anything wrong. On making my evening visit shortly before 8 o'clock, I found the patient complaining of an uncomfortable sensation about the throat, more particularly on swallowing. On examination with finger and spatula, could ascertain nothing to cause annoyance; but, upon feeling externally, found a puffy swelling on the right side of neck, extending upwards to the ramus of the jaw, and downwards over the clavicle to the thorax, terminating on the right side about the posterior axillary line, and being gradually lost on the left in front, not reaching the anterior axillary line on that side. On pressure, it gave the characteristic crackling of subcutaneous emphysema, being most marked in the supra- and infra- clavicular regions on the right side. There was no pneumothorax or dyspnœa. The only effect upon patient was what she called "a wrenched sensation about her throat." This, it appears, had come on about half-an-hour previously, whilst taking a drink of tea. Previous to this the patient had been easy and comfortable, having slept a little since her delivery. The disagreeable sensation passed away in four days; and, at the ninth day, no trace of emphysema could be found. Patient made excellent recovery. Pulse and temperature always normal.

*Professor Simpson* thought the case remarkable, inasmuch as nothing had apparently occurred during labour to cause the emphysema; and the escape of air into the cellular tissue seemed to have taken place subsequently to the labour, as it was not till some time after its termination that the emphysema was observed.

*Dr Underhill* remarked that the case differed from one recently communicated to the Society by him, and in which great dyspnœa came on during the second stage of labour.

## SESSION XXXVI.—MEETING X.

*Wednesday*, 11*th April* 1877.—Professor SIMPSON, *President, in the Chair.*

I. *Professor Simpson* exhibited a MUCOUS POLYPUS which had been attached within the canal of the cervix, and had given rise to much hæmorrhage.

II. *Dr Angus Macdonald* then read Part I. of a paper on the BEARING OF CHRONIC DISEASE OF THE HEART ON PREGNANCY AND PARTURITION. Discussion of the subject adjourned till conclusion of paper.

III. *Dr Underhill* read a CASE OF SPONTANEOUS VERSION AFTER RUPTURE OF THE MEMBRANES IN A PRIMIPARA.

*Professor Simpson* thought it was of importance to record the history of all such cases. The only difficulty in this case was to decide if labour had really begun, and the change in the presentation thus really fell under the category of spontaneous versions during parturition, or whether the change of position was due to spontaneous movement of the child, such as, commonly enough in multiparæ, at least, took place up to the period of labour. It was interesting to observe that the change had taken place from head to breech, the reverse of what most frequently happened.

## SESSION XXXVI.—MEETING XI.

*Wednesday*, 25*th April* 1877.—Professor SIMPSON, *President*, *in the Chair.*

I. *Professor Simpson* read description of an ACARDIAC FŒTUS, which had been shown to the Society at last meeting, and had now been carefully dissected and examined.

*Dr Matthews Duncan* thought this the finest specimen of an acardiac fœtus he had seen, and considered the facts brought out on dissection interesting and likely to prove interesting in the future study of the subject.

II. *Dr Angus Macdonald* then continued his paper on the BEARING OF CHRONIC DISEASE OF HEART ON PARTURITION AND PREGNANCY, and in connexion with the paper showed a HEART with a rare form of lesion—constriction of both mitral and tricuspid valves.

# Part Fourth.

## PERISCOPE.

M. DUPLAY, writing in the *Archives Générales de Médecine*, gives a very interesting review of antiseptic surgery, and comparison between Lister's and Volkmann's methods.

He begins by saying that, whereas in former times variations in the treatment of wounds depended on theories or empiricism, the antiseptic treatment of the present day is founded on the results of scientific research. By the term antiseptic here employed he refers not to Mr Lister's method alone, but to all those methods which aim at preventing putrescence of wounds and discharges. He admits that, though the French surgeons have not adopted it, Mr Lister's is the only true and perfect antiseptic method, and that it has, by its success in the hands of many surgeons of different nationalities, drawn so much attention to itself as to necessitate a review of the subject and a statement of the system as expounded by Mr Lister.

He begins his description by stating that the method of treatment is not merely a system of disinfection, but of prevention of putrescence, and then gives a very fair and thorough description of Mr Lister's mode of procedure.

Having stated the process, he begins to criticize it,—

1*st.* As to the theory. Whilst admitting that Mr Lister has abundantly proved that the putrescence of wounds and discharges is due to the action of organisms or particles suspended in the air, he considers that it is not yet sufficiently clearly shown that such putrefaction is the cause of pyæmia, erysipelas, gangrene, etc. As to the vibrios, he thinks there is nothing to show that they may not be merely concomitants of putrefaction, or even the *result* instead of the *cause.* He considers also that there is proof wanting as to the complete prevention of pyæmia by antiseptics, and he objects very strongly to the employment, in the reports of fatal cases, of such indefinite terms as anæmia, shock, and exhaustion, as causes of death.

2*d.* As to results. Dr Duplay first compares Mr Lister's published results with those of Mr Syme and Mr Spence. Here he does not hesitate to give the palm to Mr Spence. He then compares the results of those Continental surgeons who practise antiseptics with that obtained by Krönlein who uses no dressing at all. The results obtained by the latter certainly show a great superiority to the others both in the way of speedy cures and diminution of fatal cases. Dr Duplay admits that it is not fair to condemn Lister's method on the ground of Krönlein's figures, however imposing, but he omits a still stronger objection to so hasty a condemnation, namely, that the Continental surgeons who profess to follow Mr Lister's method may fail to do so perfectly, and so the system may get the blame where it does not deserve any.

3*d.* As to practical application. Dr Duplay says,—1. It is not exempt from danger. This, as we know, is not so great as was at one time supposed, and can be prevented in most cases by proper precautions. In fact, Dr Duplay himself says, "These accidents are certainly avoided if one conforms to the precepts indicated by Mr Lister." 2. The system is expensive, and requires both time and trouble in carrying out. This cannot but be, and is not denied. 3. The treatment cannot be applied by any surgeon single-handed, more than one assistant generally is necessary. This also is only too true.

From these objections (he does not mention any of the advantages) Dr Duplay argues that "experience demonstrates the superiority of Lister's method, yet it will not become generally useful until it is considerably modified (simplified) in its application."

In addressing those surgeons who have written in praise of Mr Lister's method, he says, "One ought to wait for the judgment of the future, and not indulge in premature enthusiasm." However, with all his objections to, and prejudices against the method, he cannot but admit its practical success, for he says "the secret of the success obtained by the partisans of Lister's method, is the necessity on the part of the surgeon of applying the same care and attention to all the details of the dressing till the cure is completed."

Staff-Surgeon Reid, R.N., has published a very interesting case of aneurism successfully treated by Esmarch's elastic bandage and compressor. The patient had a popliteal aneurism, which had been unsuccessfully treated by flexing the knee, and by pressure on the femoral artery by Carte's apparatus Dr Reid applied Esmarch's bandage to the whole limb from the toes upwards, passing lightly however over the aneurism, in order not to expel the blood from its sac. The elastic tubing was then wound round the limb as high up as possible, and the bandage removed. After an hour the apparatus was removed, the pulsation in the tumour was gone, and was never renewed. A Carte's compressor was substituted for Esmarch's apparatus, and was kept on at intervals for about a day. After eight months the report is, "There is a small lump in the left popliteal space. The femoral pulsates as far as Hunter's canal, below which it is occluded. Several small vessels beat round the joint, notably one about two inches from the inner edge of the patella. The patient says the one leg is as good as the other."

Dr Reid, in applying the above treatment, was acting on a principle, and with a special object, both original and essentially good. His aim was to empty the limb of blood, and keep it empty for a time, while the sac of the aneurism was allowed to retain sufficient blood to coagulate, become organized, and occlude the cavity. The whole matter depends on how long blood takes to coagulate "when in a state of stagnation in an internal cavity. Experiment as to this is wanting. But the above case seems to prove that about an hour may be sufficient."

Ordinary compression, Dr Reid thinks, only defeats its own end by establishing a collateral circulation. Even ligature of the artery above does not produce as complete stagnation in the aneurism as the treatment by Esmarch's apparatus secures. On the whole, the treatment seems well worthy the attention of surgeons, being easily applied, safe, and (in the above case) successful.

# Part Fifth.

## MEDICAL NEWS.

Professor Lister, after considerable hesitation, having made up his mind to go to London, we already hear of several candidates who are likely to come forward. The three surgeons, and the two assistant-surgeons to the Royal Infirmary, are all said to be in the field; and with so much teaching talent and experience to select from, the Crown will only be embarrassed by riches in the choice of a Professor of Clinical Surgery.

LUNACY LAWS.—So far as yet appears, the inquiry by the Select Committee of the House of Commons on the Lunacy Laws is mainly directed to the consideration of the question, whether the existing guarantees for the protection of the liberty of the subject, and of the estate of lunatics, are adequate for the purpose. Two proposals have been made by witnesses with the view of increasing these guarantees. The first is, to require that the medical certificates necessary for placing a patient in an asylum shall be granted exclusively by specialists who have made insanity their particular study; and the second, that all Chancery lunatics requiring asylum treatment shall be placed in special asylums to be built for their accommodation.

## OBITUARY.

### PETER BROTHERSTON, F.R.C.S. EDIN. 1858, SURGEON, Alloa.

THE readers of this Journal, and especially the older ones, must have noticed with regret the death of Mr Brotherston of Alloa. It is not too much to say, that among the provincial surgeons of Great Britain he stood in the highest rank; whether we test his excellence by his knowledge of the profession he loved so well, or his skill in the practical application of that knowledge, when his judgment led him to the conclusion that manual interference was necessary. No wonder, then, that, after a painstaking, hardworking busy service of thirty-five years in Alloa and the surrounding country, his sudden death should have caused a universal feeling of deep regret among those who knew and esteemed him. In Alloa we have been told the scene at the funeral was most impressive, all from the highest to the humblest of the district doing honour to his obsequies—and no wonder, for he was a man that would as soon have given the benefit of his valuable services to the poorest in the land as to the most affluent. A difficult case, requiring special surgical knowledge, was the main object of his ambition, and the annals of the Cottage Hospital which he was chiefly instrumental in founding, and which he worked gratuitously, can show the amount of unremunerated labour he performed from the pure love of his profession.

He was born in 1820, and was the son of the much-esteemed minister of the Established Church in Alloa. When about sixteen years of age he was apprenticed to Dr M'Gowan of Alloa, who was in the way of seeing a good deal of surgical practice among the collieries, and it has been suggested that while with him he acquired the great love of surgery afterwards manifested. It possibly may be so, but at the same time it must be borne in mind that he studied in Edinburgh, and that his intense admiration for the late Professor Syme was one of the most marked features in his subsequent career. It is well known that the Professor entertained a high opinion of his surgical abilities, and in later years, one of the most welcome and honoured visitors at his Infirmary Clinique was Peter Brotherston.

There is no doubt that, while acquiring fame as a surgeon was from the first the highest object of his ambition, his active mind had room for other pursuits; and the archives of the Archæological Society of Alloa, started in 1863, and of which he was chosen vice-president in the first year of its existence, can show how useful he could make himself in that department of science. We shall print below his contributions to that Institution. His illustrations in natural history were chiefly procured by the deadly accuracy of his gun. Mr Brotherston was not only a good shot but a good angler; and his friends can look back with a melancholy pleasure to the happy ploys they had in his company, where the natural beauty of the scenery and the capture of the finny tribe were much enhanced, as regards enjoyment, by the vivacity and quaintness of his remarks. Though he completed his course of study for his degree at the Edinburgh University, he did not go in for it, but took instead the diploma of the Royal College of Surgeons of England, and in 1858 became a Fellow of the sister college in Edinburgh. He was not a voluminous writer, but his contributions, chiefly to this Journal, were practical in an eminent degree. In 1856 he was one of the first to revive in Scotland the operation of excision of the knee-joint, which had fallen into disuse, but which was about that time strongly urged on the profession by Mr Jones of Jersey. His two cases were successful, and he brought one of them down to the Medico-Chirurgical Society of Edinburgh for exhibition. Besides performing, and performing well, a large number of the major operations in surgery, his skill as an accoucheur was generally acknowledged, and in the surgical department of that special branch of medicine his hands were always ready to do what his knowledge and great experience dictated. Two papers illustrate this: (1) "Case of post-mortem Cæsarean section, child saved" (*Edinburgh Medical Journal,* 1868); (2) "Case of Cephalotripsy after turning" (*Ibid.* 1874).

In what we have hitherto said we have abstained from intruding on the privacy of his domestic life, but we may be permitted to remark, that from what we have observed, his amiable and affectionate disposition endeared him to all who were bound to him by family ties. There was a natural simplicity about him, which contrasted forcibly with the shrewdness he so often displayed. Though not quite an Edwards, perhaps, in the firm fixity of pursuit, it is notorious that from early boyhood he showed a warm predilection for the charms of natural history, and acquired in early life a very respectable museum by his own labours and prowess. In later years his extensive practice in a more useful profession interfered greatly with his earlier tastes; but even with impaired health he had yet found time to commence putting together for the benefit of the Archæological Society a paper entitled—"Natural history researches at San Remo."

The mention of this place naturally brings us to the beginning of the end.

About four years ago he began to be subject to occasional attacks of asthma, which incapacitated him for the time from work, and which evidently acted on his nervous system, for his friends noticed him to be frequently oppressed in spirits, and affected occasionally indeed even to tears. After a severe illness of this nature,—asthma and great depression—Dr Warburton Begbie recommended a complete relaxation from work and a winter spent at San Remo. He went there on the 20th January, and returned on the 24th May 1875 much improved in health, and hopeful of a complete change for the better; but after a time the old symptoms began to show themselves, and it was evident to all around him that he had serious apprehensions about himself. His last appearance among his Edinburgh friends was at the Harveian Festival of the 12th of April, where the depression in spirits of one who used to be among the most hilarious was very generally remarked. We firmly believe that a presentiment of his approaching end was at that time preying on his mind. Eight days afterwards, on the morning of the 20th April, he was found dead in his bed, his head resting in a placid manner on his hand. Subsequent examination, we are told, showed extensive fatty degeneration of the heart.

We must sum up this imperfect sketch by saying that his native town, Alloa, has lost one of its most useful and valued citizens; his family, a kind and loving husband and father; his friends, a true and most obliging associate; and medical science, one of its most ardent votaries.

The following is a list of Papers read by the late Mr Brotherston to the Alloa Society of Natural Science and Archæology:—Seven papers on "The Birds of Clackmannanshire," in 1863, 1865, 1866, 1867, 1869, 1870, and 1873. "The Rarer Fishes found in the Forth," in 1864. "Notes on recently captured Whales in the Forth, near Alloa," in 1865. "Our Holiday in Harris," in 1870.

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## CORRESPONDENCE.

*To the Editor of the Edinburgh Medical Journal.*

United College, St Andrews, *May* 1877.

Sir,—In the May issue of your Journal appears a review of my "Ancient Life-History of the Earth," which contains certain misleading or absolutely incorrect statements, and upon which, therefore, I feel entitled to make one or two brief remarks:—

In the first place, as regards the *illustrations* of the work, your reviewer says, "The woodcuts which appeared in the Palæontology (*i.e.*, my 'Manual of Palæontology'), supplemented by several new figures, are used in the illustration of the work before us;" and, again, "The work, it should be mentioned, is copiously illustrated; the illustrations derived from D'Orbigny's 'Cours Elémentaire de Paléontologie,' used, as already mentioned, in the author's former treatise, being again employed in the present instance." Now, I would not presume to say how the reviewer may understand the word "*several*," but I think outsiders may easily form their own opinion as to the honesty and fairness of the above statements, when I simply say that this word is employed to cover over one hundred engravings, embracing two hundred and ninety-five figures, *none* of which were used in my "Manual of Palæontology," and all of which, with the exception of two, or perhaps three, were either engraved especially for the present work, or were taken from original palæontological memoirs of my own; whilst the second statement does not recognise the existence of any fresh illustrations at all.

In the second place, to the statement that the present work is merely a "*rechauffée*" of my "Manual of Palæontology," having neither time nor inclination to enter into details, I must simply oppose my total and explicit denial. The section entitled "Historical Palæontology" in my "Manual" has been incorporated in the present work (with many enlargements and modifications), simply upon the ground that it is to be omitted from the new edition of the former work, now in preparation. Chapter V., and the few other scattered passages alluded to by your critic, together with some other short passages not mentioned by him, have also been taken from my "Manual," because the two works treat of the same subject, the one technically, and the other elementarily; and these particular passages required no alteration to suit them for the less technical work. I repeat, however, that in no sense whatever is my present work a mere repetition of my former and larger one, or even an abridgement of it; but it is, on the contrary, to all intents and purposes, an entirely new production. The final decision of this question I cheerfully leave in the hands of the public and of all impartial critics.

The singularly few scientific strictures of your reviewer, for reasons which every palæontologist will understand, require no answer from me. I would just remark, however, that his demurrer to my "unqualified assertion," contained "in the last sentence" of my work, that "to Palæontology alone we must look for the solution of the problem dealing with *the origin of living beings*" (the italics are mine), is, to say the least of it, somewhat premature, seeing that no such assertion—nor anything that human ingenuity could twist into such an assertion—occurs in the last sentence of my work, or anywhere else throughout its pages.—Yours, etc.,

H. ALLEYNE NICHOLSON.

[THE above remarks of Professor Nicholson resolve themselves into a complaint that I have misrepresented the number of new illustrations in his recently-published volume, and that I have overstated the amount of his indebtedness to his former work, entitled, "A Manual of Palæontology." With regard to the first of these points, it may be stated that, out of the 270 illustrations contained in the work under discussion, 164, at least (including D'Orbigny's illustrations), have been borrowed from the author's "Palæontology" and "Zoology." Some of these cuts, it may be mentioned, are of singularly venerable nature, and include what a reviewer in a contemporary when treating of Dr Nicholson's work, terms "our friend the mammoth with wooden feet." I do not know what Professor Nicholson's opinion may be regarding the epithet which should be applied to the illustration of a work in which the proportion of old to new cuts is as 164 to 106; but I am inclined to believe that my remarks concerning the use of old material in this respect will be fully justified in the minds of all reasonable readers. Professor Nicholson is singularly unfortunate in his statements regarding the adaptation of matter from his "Palæontology." He actually concedes, and in the most effective manner illustrates against his own case, the point at issue. In the one breath he maintains that the present work is "to all intents and purposes an entirely new production." In the next we are informed that a whole section from the "Palæontology" has been "incorporated" in the avowedly "new production,"—along with, we may add, numerous other passages from the latter work, a very few of which were indicated in the review. After the author's own and direct admission of the manner in which the "Palæontology" has served a second turn in the compilation of the present volume, nothing further need be said on this point, save to remark, that in fairness to the public to whom he appeals, Dr Nicholson should have been as explicit in stating his indebtedness to his "Palæontology" in the pages of the work now under review, as he is in the above communication. The case may, in fact, to quote the author's own words, be safely left "in the hands of the public and of all impartial critics." The last point to which reference is made by Dr Nicholson concerns my remark that he has made an unqualified assertion in the last sentence of his work, where, in speaking of the nature, extent, laws, etc., of evolution (this term, apart from quibbling, indicating a theory of the origin of new species), he says—"In the successful solution of this problem—if soluble with the materials available to our hands—will lie the greatest triumph that Palæontology can hope to attain," etc. This, to my mind, is a bare "assertion;" and it is further "unqualified" by a single statement to show that the author believes in or hopes for any other than a palæontological solution of the evolutionary problem. How my demurrer can be termed "premature," or why Dr Nicholson fails to understand the meaning of what he himself writes, requires, I frankly admit, a larger share of "human ingenuity" than I possess. The present is not the first occasion on which Dr Nicholson has thought fit to criticise his critics. I freely admit the right of every author to reply to any statements of a reviewer through which he may reasonably feel aggrieved. But the reviewer has, at the same time, a right to expect sound argument and not mere cavilling. On the next occasion on which Dr Nicholson thinks fit to reply to a reviewer in the terms used in his letter, I trust he will succeed in justifying his use of the words "honesty," "fairness," etc., so prominently displayed in his present communication, as fully as he has succeeded in being both querulous and self-contradictory.—THE REVIEWER.]

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*To the Editor of the Edinburgh Medical Journal.*

SIR,—As I had not an opportunity of fully replying to the gentlemen who made remarks on the paper that I lately read before the Medico-Chirurgical Society, would you insert the following paragraphs in your Journal?

Professor Sanders, in the course of his observations, used a sort of *a priori* argument in stating that no sound can be produced by contraction of the auricles. But how can we determine whether or not a sound is produced by contraction of the auricles except by listening to the action of the denuded heart in a living animal? And, in the course of our experiments, we have so frequently heard the second sound of the heart produced by contraction of the auricles, that the fact is placed beyond every possibility of a doubt. But it is admitted that the pre-systolic sound is produced by contraction of the auricles, which settles the matter so far as relates to the power possessed by the auricles of producing a sound during their contraction. And at what time is this sound produced? It is admitted that it is during the diastole of the ventricle that the auricles contract and bring the ventricle to the point of distension. And you say that the closing of the aortic valves, which produces a sound, takes place synchronously with the diastole of the ventricle. We have, then, in this case of morbid action—constriction of the mitral orifice—two sounds produced, according to your view, during the diastole of the ventricle, and one during its systole, for the ventricular contraction, as is well known, produces a sound.

But we must explain these phenomena according to the principle on which the action of the heart is maintained. We have stated that the first sound of the heart consists of that of the contraction of the ventricle and reaction of the aorta, which take place together in producing the full blood-wave. But when the action of the heart becomes much weakened or irregular from disease, this sound is heard with a slight break in it, which you term the first and second sounds of the heart; but in the normal and vigorous action it is always united, and constitutes one prolonged systolic sound—the first sound of the heart. And the second sound, produced by the vigorous contraction of the auricles, immediately follows the first. But here, the auricles being later in contracting, the sound which they produced is brought more immediately into connexion with the ventricular sound. Hence you have three sounds instead of two.

Dr Balfour, in the course of his remarks, endeavoured to show that much difficulty must be experienced in determining the cause of the first sound of the heart in a case where two tiers of semilunar valves existed at the origin of the aorta—a remark totally inapplicable to the subject; for this difficulty did not obtain in the case of the alligator terrapin, where there is only a single tier of semilunar valves. We had, some time since, presented to Dr Balfour the heart of a sturgeon fish, in which class of animals muscular fibres are continued from the ventricle along the aorta for an inch or more; and, in adaptation to the increased power of the aorta, two tiers of semilunar valves are placed at its origin, evidently, as we stated, to resist the increased force of the blood recoiling against them as the aorta reacts or contracts to produce the full blood-wave. But we made no allusion to a sound being heard, or attempting to hear one.

We had previously shown the same preparation to some eminent physicians in London, and also in Paris; but these gentlemen had no difficulty in comprehending the meaning of the language we used.

The experiments we performed were too carefully conducted, and too accurately tested, to be liable to Dr Balfour's objection. And we surely have had much better opportunities of investigating this subject in the denuded heart of living animals, both in cold and warm-blooded, than those with which physicians are generally favoured; and the circumstance that the doctrines we advance are new, and calculated to produce a change in views that have been long accepted, ought not to militate against them. The question to be determined is, Are these statements based on facts? But we need not *re-enter* on the proposition that we have so fully illustrated, and which is acknowledged by mathematicians, philosophers, and other scientific men,—that you cannot propel a wave of fluid along an elastic tube like the aorta without the distended walls reacting and closing the valves at the orifice to produce the full wave. Whatever objections you may urge, you cannot deny this important law in physical science; and as the ventricle, on being fully contracted—shortened in its fibres—exerts a strain or slight retraction on the base of the distended aorta that produces its simultaneous reaction, the sound produced in closing the aortic valves is a systolic sound, and identified with the first sound of the heart, and the second sound is produced by the contraction of the auricles, and connected with the diastole of the ventricle. G. PATON.

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*Meeting* iv., 17*th January* 1877.—Exhibition of patients, specimens, etc., by Messrs Annandale and Chiene and Dr Wyllie; paper by Dr Matthews Duncan on complete inversion of the uterus, with discussion by Drs Gillespie, Keiller, Charles Bell, A. Macdonald, and M. Duncan; paper by Mr Annandale entitled note on Esmarch's operation for permanent closure of the jaws, 836–841.

*Meeting* v., 7*th February* 1877.—Case of croup by Dr Bruce; discussion on Dr Craig's paper on the therapeutics of aloin by Drs Gillespie, Keiller, and Craig; discussion on Dr Batty Tuke's paper on the diagnosis of general paralysis of the insane by the President, Dr A. Robertson, Mr Bell, Drs Keiller and Tuke; discussion on Mr Annandale's paper on penetration of the brain through the orbit by Dr Gillespie, Mr Bell, Dr A. Robertson, and Mr Annandale, 934–938.

*Meeting* vi., 7*th March* 1877.—Exhibition of patient, pathological specimens, etc., by Messrs Annandale, Bell, and Chiene, and Drs A. Robertson, G. W. Balfour, and B. Bramwell; discussion on Dr Turnbull's case of colica pictonum, and on Dr Paton's paper on the action of the heart by Dr Gillespie, Mr Annandale, Prof. Sanders, Dr Wyllie, Mr Bell, Drs Carmichael, Miller, Keiller, and Turnbull, and Drs Balfour, Smart, and Bramwell, 1024–1032.

*Meeting* vii., 4*th April* 1877.—Exhibition of patients; pathological specimens, etc., by Messrs Annandale and Hamilton, and Drs Argyll Robertson and Bramwell; discussion on Mr Annandale's paper on internal hæmorrhoids, by Drs Gillespie, Duncan, Cadell, Bruce, Carmichael, and Messrs Chiene and Bell; and on Dr Cadell's paper on varieties of infecting chancre, by Drs Gillespie, Duncan, Miller, and Cadell, and Messrs Chiene and Annandale, 1120–1128.

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*Meeting* v., 24*th January* 1877.—Exhibition of specimens by Dr Matthews Duncan; discussion on Dr Cuthill's paper on the action of the ergot of rye by Professor Simpson, Drs A. Jameson, J. Young, Craig, Kennedy, Keiller, Bruce, Matthews Duncan, and Cuthill; discussion on Dr Finlay's case of triplets, by Drs Craig, Underhill, Gordon, Simpson, and Keiller, 938–942.

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